THE
UNIVERSE OF STARS.

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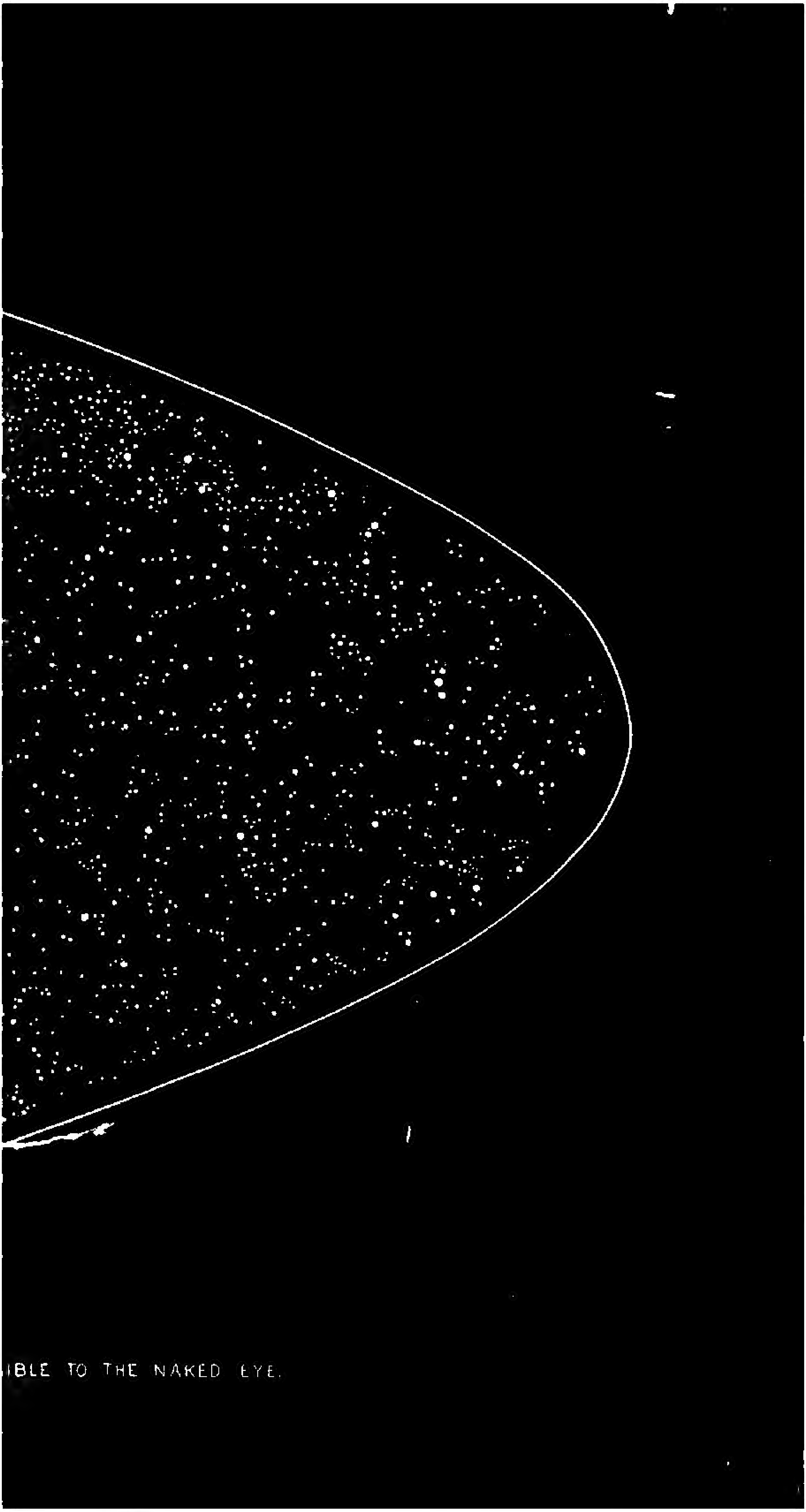

IBLE TO THE NAKED EYE.

THE

UNIVERSE OF STARS:

PRESENTING RESEARCHES INTO AND
NEW VIEWS RESPECTING

THE CONSTITUTION OF THE HEAVENS.

XXVI 4 C-12

BY

RICHARD A. PROCTOR,

AUTHOR OF 'THE SUN,' 'THE MOON,' 'SATURN,' 'OTHER WORLDS THAN OURS,'
'SCIENCE BYWAYS,' 'THE BORDERLAND OF SCIENCE' ETC.

SECOND EDITION.

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TO

JOHN COUCH ADAMS
AND
TO THE MEMORY OF
THE LATE
URBAIN J. J. LEVERRIER

THE TWO ASTRONOMERS
WHO FIRST OF ALL MEN DETERMINED
THE PLACE OF AN AS YET UNKNOWN PLANET
BY ANALYSING THE PERTURBATIONS
OF A KNOWN ONE

THIS EDITION IS DEDICATED
BY
THE AUTHOR

FROM PREFACE to FIRST EDITION.

THE ESSAYS on the Universe present, very nearly in chronological order, my researches into and gradually advancing views respecting the constitution of the Universe. With the exception of some papers which have appeared in my 'Essays on Astronomy,' all my original researches into this subject are given in the present volume.

I believe that my account of the results obtained by Sir W. Herschel during his long and arduous labours in the study of the star-depths is more accurate than any yet presented.

In this volume I give full copies of all my maps and charts of the star-depths, except in the case of my large chart of 324,000 stars, sections from which only are given.

Plate VII. is by Mr. Sidney Waters, F.R.A.S., and is, in my judgment, a most valuable contribution to uranography.

RICHARD A. PROCTOR.

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THE UNIVERSE.

STAR-STREAMS.

To those who rightly appreciate its meaning, the Milky Way is the most magnificent of all astronomical phenomena. However opinions may vary as to the configuration of the star-streams composing this object, no doubt now exists among astronomers that the Milky Way consists really of suns, some doubtless falling short of our own sun in brilliancy, but many probably surpassing it. Around these suns, we may fairly conceive, there revolve systems of dependent orbs, each supporting its myriads of living creatures. We have afforded to us a noble theme for contemplation, in the consideration of the endless diversities of structure, and of arrangement, which must prevail throughout this immensity of systems.

I propose to examine what is known of this marvellous object, and to present some considerations which appear to me to have an important bearing on the views we should form of its structure.

The galaxy traverses the constellation Cassiopeia. Thence it throws off a branch towards *a* Persei, prolonged faintly towards the Pleiades. The main stream, here faint, passes on through Auriga, between the feet of Gemini and The

Bull's horns, over Orion's club, to the neck of Monoceros. Thence, growing gradually brighter, the stream passes over the head of Canis Major, in a uniform stream, until it enters the prow of Argo, where it subdivides. One stream continues to γ Argus, the other diffuses itself broadly, forming a fan-like expanse of interlacing branches which terminate abruptly on a line through λ and γ Argus. Here there is a gap, beyond which the Milky Way commences in a similar fan-shaped grouping, converging on the brilliant (and in other respects remarkable) star, η Argus. Thence, it enters the Cross by a narrow neck, and then directly expands into a broad, bright mass, extending almost to *α* Centauri. Within this mass is a singular cavity known as the Coal-Sack. At *α* Centauri the Milky Way again subdivides, a branch running off at an angle of 20°, and losing itself in a narrow streamlet. The main stream increases in breadth, until, 'making an abrupt elbow,' it subdivides into one continuous but irregular stream, and a complicated system of interlacing streams covering the region around the tail and following claw of Scorpio. A wide interval separates this part of the galaxy from the great branch on the northern side, which is seen in plates I., II., and VI., terminating close on β Ophiuchi.

The main stream, after exhibiting several very remarkable condensations, passes through Aquila, Sagitta, and Vulpecula to Cygnus. In Cygnus there is a 'confused and patchy' region, marked by a broad vacancy, not unlike the Coal-Sack. From this region there is thrown off the offset to β Ophiuchi already mentioned; the main stream is continued to Cassiopeia.

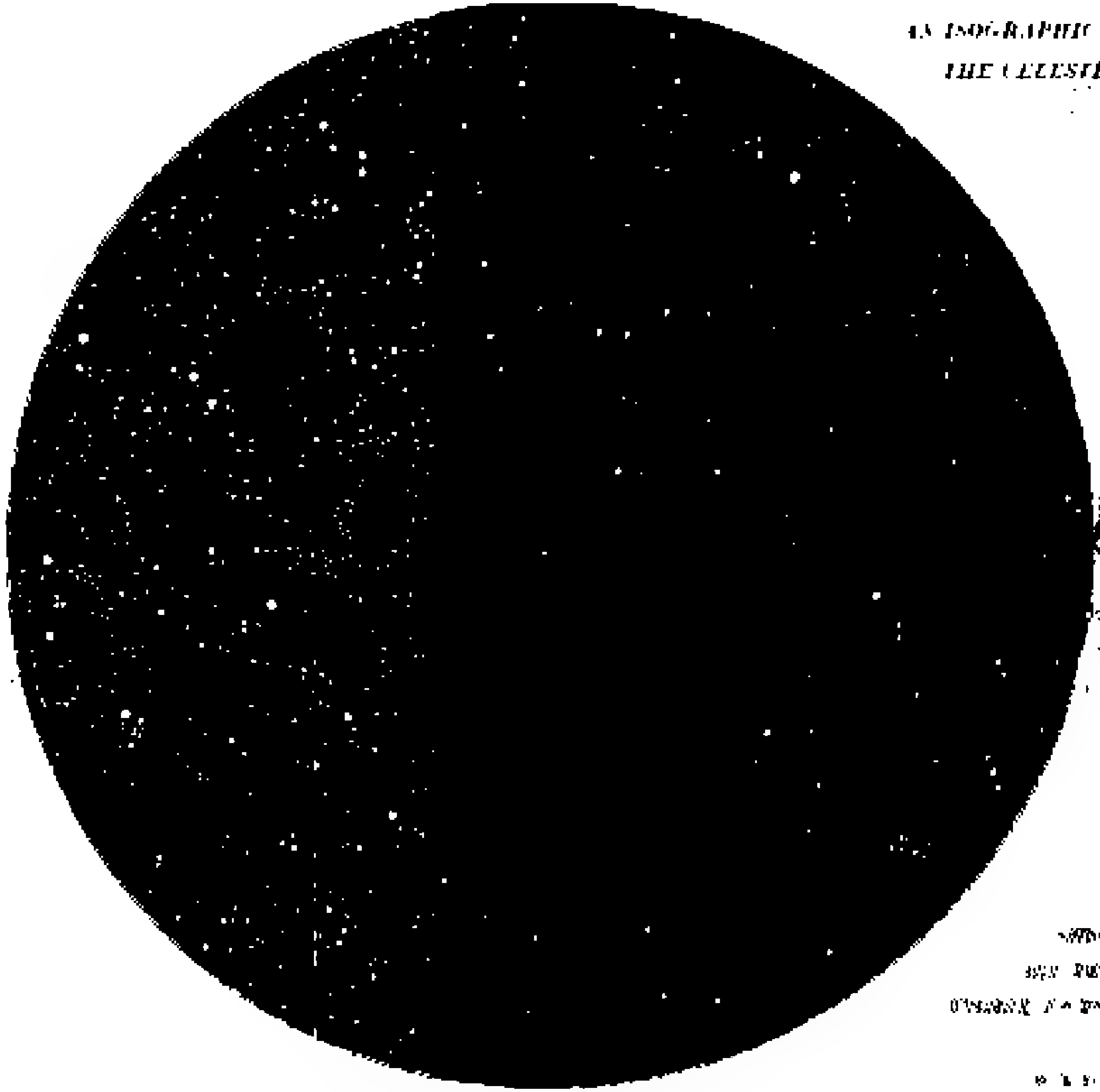
There only remains to be noticed 'a considerable offset or protuberant appendage,' thrown from the head of Cepheus directly towards the pole.

A word as to the changes in the appearance and position of the Milky Way from month to month, at any given hour. Selecting ten o'clock in the evening, as the most convenient

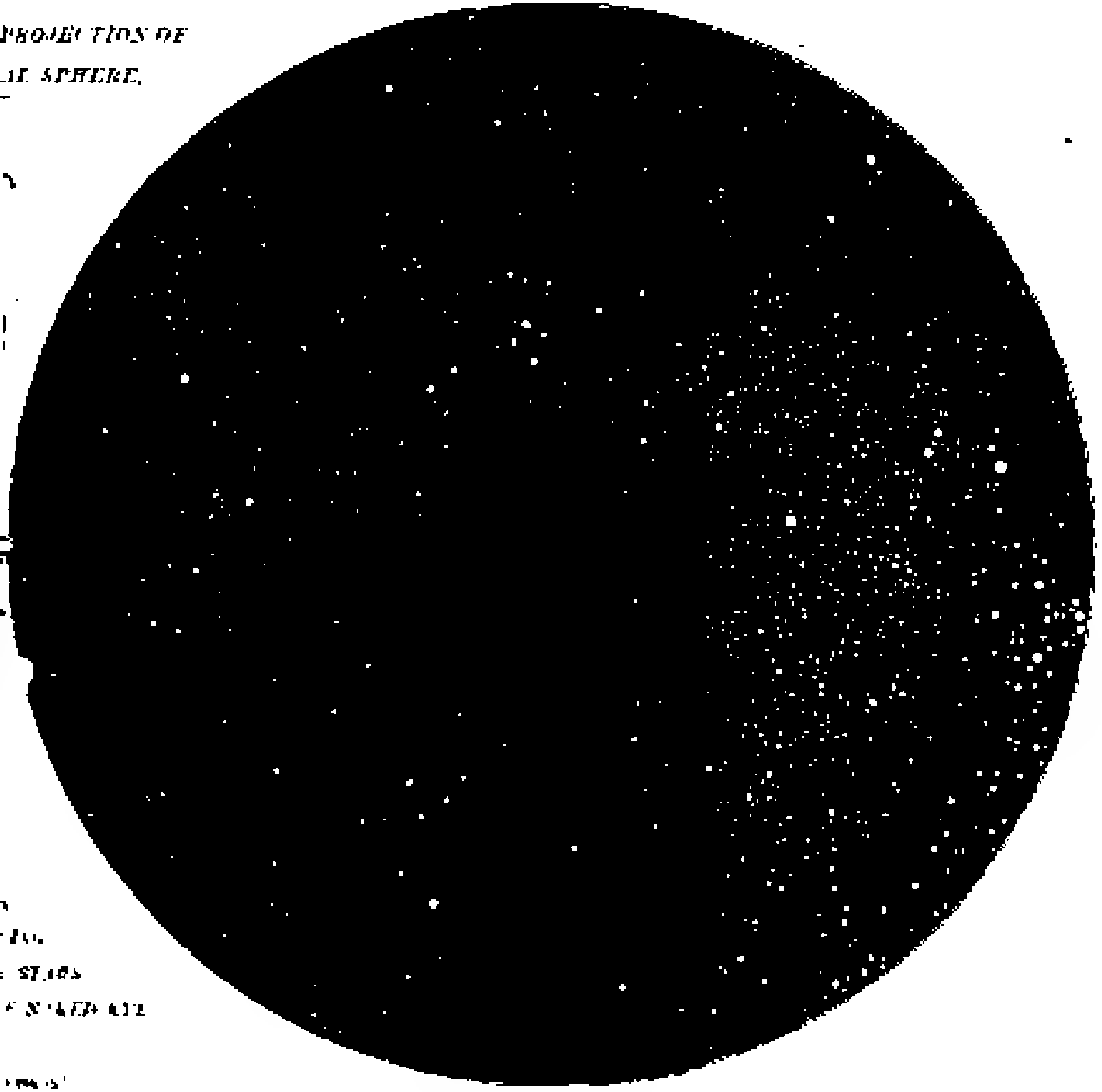
No 1

AN ISOGRAPHIC PROJECTION OF
THE CELESTIAL SPHERE,

I.—NORTHERN HEMISPHERE



II.—SOUTHERN HEMISPHERE



SHOWING
ALL THE STARS
VISIBLE TO THE NAKED EYE

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hour, we have the following variations of configuration :—At the winter solstice the Milky Way passes nearly through the zenith, crossing the horizon towards the south-east and north-west. Its brightest part is low down towards the last named quarter, where the constellation Cygnus lies close to the horizon. One month later, the Milky Way crosses the horizon towards the south-south-east, and north-north-west, and is bowed some 20° from the zenith towards the west-south-west. Cygnus is now half-set. Yet another month, and the Milky Way is found crossing the northern and southern points of the horizon, and bowed about 40° from the zenith towards the west—Cygnus more than half-set. At the Vernal Equinox, the Milky Way crosses the horizon towards the south-south-west, and north-north-east, and is bowed upwards of 50° from the zenith towards the west-north-west. One month later (that is about April 20), we find the Milky Way crossing the horizon towards the west somewhat southerly and towards the east somewhat northerly, and only raised about 20° above the northern horizon. It is now easy to follow the remaining changes without special comment. The eastern end travels southwards, the western northwards, along the horizon, the central part approaching the zenith, just as hitherto it has been seen to leave the zenith. The bright parts in Cygnus and Aquila are more and more favourably seen as they approach the zenith, being best seen in July and August (at 10 P.M.). About this time we see the southern portion of the Milky Way somewhat beyond Antares (the heart of Scorpio), whereas six months before the greatest range on the opposite side (including nearly the whole of the gap in Argo) had been visible. At the end of October the Milky Way (at 10 P.M.), is seen vertically overhead, and crossing the horizon towards the east and west quarters, the western half being the most conspicuous.

Galileo was the first to prove, though earlier astronomers had entertained the notion, that the Milky Way is composed of a vast number of stars, crowded closely together. But no attempt was made to offer a theory of its structure until, in 1754, Thomas Wright, in his 'Theory of the Universe,' propounded views closely according with those entertained later by Sir W. Herschel. Wright, having examined a portion of the galaxy with a reflecting telescope, only one foot in focal length, came to the conclusion that our sun is in the midst of a vast stratum of stars; that it is when we look along the direction in which this stratum extends, that we see the zone of light constituting the Milky Way; and that as the line of sight is inclined at a greater and greater angle to the mean plane of the stratum, the apparent density of the star-grouping gradually diminishes.

But it is to Sir W. Herschel, and the supplementary labours of Sir J. Herschel, that we owe the more definite views now commonly entertained respecting the Via Lactea. The elder Herschel, whose nobly speculative views of nature were accompanied by practical common sense, and a wonderful power of patient observation, applied to the heavens his celebrated method of gauging. He assumed as a first principle, to be modified by the results of observation, that there is a tolerable uniformity in the distribution of stars through space. Directing his twenty-feet reflector successively towards different parts of the heavens, he counted the number of stars which were visible at any single view. The field of view of this reflector was 15′ in diameter, so that the portion of the sky included in any one view was less than one-fourth of that covered by the moon. He found the number of stars visible in different parts of the heavens, in a field of view of this size, to be very variable. Sometimes there were but two or three stars in the field; indeed, on one occasion he counted only three stars in four fields.

In other parts of the heavens the whole field was crowded with stars. In the richer parts of the galaxy as many as 400 or 500 stars would be visible at once, and on one occasion he saw as many as 588. He calculated that in one quarter of an hour 116,000 stars traversed the field of his telescope, when the richest part of the galaxy was under observation. Now, on the assumption above-named, the number of stars visible when the telescope was pointed in any given direction was a criterion of the depth of the bed of stars in that direction. Thus, by combining a large number of observations, a conception—rough, indeed, but instructive—might be formed of the figure of that stratum of stars within which our sun is situated.

One section of the galactic nebula, as determined from Herschel's observations, is given in fig. 1. The projections

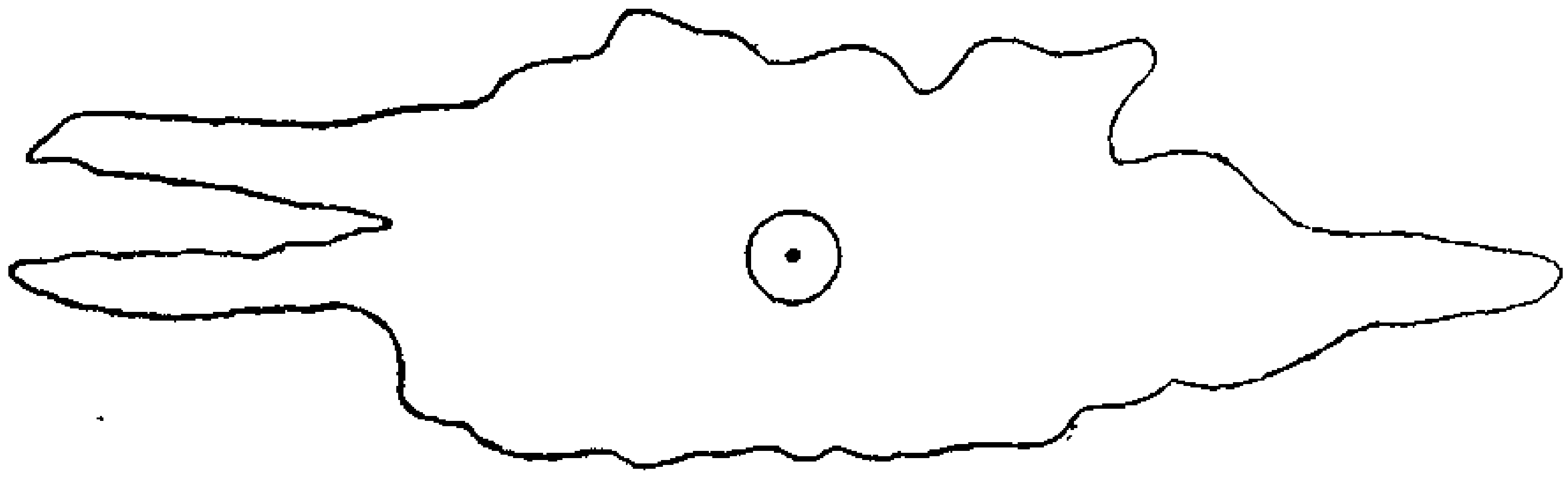

FIG. 1.

extending to the left correspond to those portions of the particular great circle considered, which cross the double part of the Milky Way; the opposite projection represents the portion crossed by the single stream; while the comparative flattening of the central part indicates the gradual diminution of star-density in directions removed from the galactic zone. It is, of course, to be understood that Herschel was far from supposing that such a figure correctly represented the figure of the section. He looked upon it as affording but a rough indication of the true figure. He had indeed noticed, so early as 1785, that there is a tendency in the Milky Way to cluster around definite regions of the heavens; and he saw that the fact of such clustering was sufficient to account for many irregularities of the figure, quite irrespectively of the absolute extent of the Milky Way in space. If we are looking from a height at the lights of a large town, we may fairly assume that a row of many lights very closely ranged, lies at a greater distance from us than another row containing lights more widely dispersed, *if* we have reason to suppose that throughout all the streets of the town the lights are separated by distances approximately equal. But if we have reason to suspect that there are some streets lighted more fully than others, the inference would be no longer valid. And again, Herschel suspected that there are stars so large as to bear a sort of sway among other stars by superior attractive influence. Here, then, was another element of difficulty, since it becomes clear (1) that the brilliancy of a star is no positive evidence of proximity; and (2) that there may be (besides the obvious clusterings already considered) laws of systematic distribution, which might largely modify the evidence afforded by star-gauging. For instance, returning to the illustration given above, if we have reason to suspect that there are many lights of superior brilliancy, in some parts of a town, and that further there are in some streets laws of arrangement among the lights, or that there are irregularities of surface-contour, which produce here and there a greater or less foreshortening than would result on a level ground, we should have to make allowance for these points in attempting to form an estimate of the distances at which the different parts of the town are removed from us.

Still, the results obtained by Sir W. Herschel have very properly been accepted as affording general evidence of high value.

Sir J. Herschel, during his residence at the Cape of Good

Hope, carried out an extensive series of observations of the southern heavens. Applying his father's method of gauging, with a telescope of equal power, he obtained a result agreeing, in a most remarkable manner, with those obtained by Sir William Herschel. It appeared, however, that the southern hemisphere is somewhat richer in stars than the northern, a result which has been accepted as indicating that our system is probably somewhat nearer the southern than the northern part of the galactic nebula. Moreover, Sir J. Herschel was led to believe that the sidereal system forms a cloven flat ring rather than a disc.

Combining the results obtained by the two Herschels, we should assign to the stratum of stars a figure somewhat resembling that of the solid cloven disc exhibited in fig. 2. This, being a side view, gives the figure of the imaginary section which appears so often in works on astronomy. In fig. 1 the small circle near the centre indicates the probable

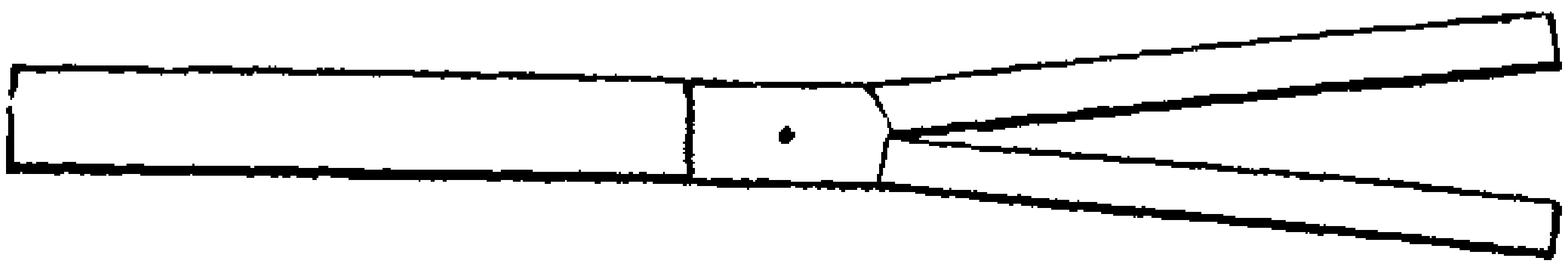

FIG. 2.

extent (on the theory in question) of the sphere within which stars down to the fifth and sixth magnitude may be supposed to be included.

The main difficulties in attempting to form an estimate of the real configuration of the galactic system are those which have been already mentioned. Have we evidence confirming or disproving (1) the tendency to clustering suggested by the elder Herschel, (2) the possible variability among star-magnitudes, and (3) the action of influences exerted by large stars in guiding or swaying others? It appears to me that there are indications of a very obvious and important character, which have been either altogether unnoticed, or much less noticed than they deserve. In considering these indications, I would refer the reader chiefly to Plates I., II., and VI. For portions of the evidence cannot be thoroughly understood without reference to star-maps, in which (at a single view, if possible) the course of the Milky Way is exhibited in a manner which enables us at once to determine its relations to the constellations. It may seem (and, indeed, on the assumption of any approach to uniformity in the true magnitudes, or distribution of stars, it must necessarily be) a very imperfect method to refer to star-maps including only the first six magnitudes.[1] It is obvious that if our sun is placed within such a stratum as is exhibited in figs. 1 and 2, no evidence whatever as to the structure of that stratum can be afforded by considering the comparatively few stars included within the small central circle. But it is this very fact on which I wish to dwell. If any connection *should* appear between the configuration of our galaxy, and the arrangement of stars which are assumed to be much nearer to us than the Milky Way, it will be obvious that we must somewhat modify the views illustrated by these figures.

Now, taking Plates I., II., and VI., I think one can trace a connection between the stars there depicted, and that stream of nebulous light which the view we are examining teaches us to consider as at an enormous distance beyond those stars. In the northern portion, perhaps, the connection is not very remarkable. We see that a large number of the brighter stars lie on or near the Milky Way, but the relation is not so marked that we can regard this arrangement as positive evidence of aggregation. However, I think no one who has attentively examined the glories of Orion, the richly-jewelled Taurus, the singular festoon of stars in Perseus, and the closely-set stars of Cassiopeia, but must have felt that the association of splendour along this streak of the heavens is not wholly accidental. The stars here seem to form a system, and a system which one can hardly conceive to be wholly unconnected with the neighbouring stream of the Milky Way. But in the southern portion the arrangement is yet more remarkable and significant. From Scorpio, over the feet of the Centaur, over the keel of Argo, to Canis Major, there is a clustering of brilliant stars, which it seems wholly impossible not to connect with the background of nebulous light. It is noteworthy, also, that this stream of stars merges into the stream commencing with the group of Orion already noticed. Nor is this all. It is impossible not to be struck by the marked absence of bright stars in the region of the heavens between Algol, Crux, and Corvus. One has the impression that the stars have been attracted towards the region of the stream indicated, so as to leave this space comparatively bare.

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[1] In the paper as it originally stood reference was made to stars down to the fifth magnitude only. The character of the evidence is not affected by the change.

Now, this last circumstance would appear less remarkable if the paucity of stars here noticed were common also in parts of the heavens far removed from the Milky Way. But this is not the case. Beyond this very region, which we find so bare of stars, we come upon a region in which stars are clustered in considerable density, a region including Crater, Corvus, and Virgo, with the conspicuous stars Algores, Alkes, and Spica. But what is very remarkable, while we can trace a connection between the stream of bright stars over the Milky Way, and the stream of nebulous light in the background, it is obvious that the two streams are not absolutely coincident in direction. The stream lies on one side of the Milky Way near Scorpio, crosses it in the neighbourhood of Crux, and passes to the other side along Canis Major, Orion, and Taurus. Does the stream return to the Milky Way? It seems to me that there is clear evidence of a separation near Aldebaran, one branch curving through Auriga, Perseus, and Cassiopeia, the other proceeding (more nearly in the direction originally observed) through Aries (throwing out an outlier along the band of Pisces), over the square of Pegasus, and along the streams which the ancients compared to water from the urn of Aquarius (but which in our modern maps are divided between Aquarius and Grus). The stream-formation here is very marked, as is evident from the phenomenon having attracted the notice of astronomers so long ago. But modern travels have brought within our ken the continuation of the stream over Toucan, Hydrus, and Reticulum (the two latter names being doubtless suggested by the convolutions of the stream in this neighbourhood). Here the stream seems to end in a sort of double loop, and it is not a little remarkable that the Nubecula Major lies within one loop, the Nubecula Minor within the other. It is also noteworthy that from the foot of Orion there is another remarkable stream of stars, recognised by the ancients under the name of the River Eridanus, which proceeds in a sinuous course towards this same region of the Nubeculæ.

Having thus met with evidence—striking at least, if not decisive,—of a tendency to aggregation into streams, let us consider if, in other parts of the heavens, similar traces may not be observable. We traced a stream from Scorpio towards Orion, and so round in a spiral to the Nubeculæ. Let us now return to Scorpio, and trace the stream (if any appear) in the contrary direction. Now although over the northern hemisphere star-streams are not nearly so marked as over the southern, yet there appears a decided indication of stream-formation along Serpens and Corona over the group on the left hand of Bootes to the Great Bear. A branch of this stream, starting from Corona, traverses the body of Bootes, Berenice's Hair, the Sickle in Leo, the Beehive in Cancer, passing over Castor and Pollux in Gemini, towards Capella.

A branch from the feet of Gemini passes over Canis Minor, along Hydra (so named doubtless from the obvious tendency to stream-formation along the length of this constellation), and so to the right claw of Scorpio.

One other remarkable congeries of stars is to be mentioned. From the northern part of the Milky Way there will be noticed a projection towards the north pole from the head of Cepheus. This projection seems to merge itself in a complex convolution of stars forming the ancient constellation Draco, which doubtless included the ancient (but probably less ancient) constellation Ursa Minor. After following the convolutions of Draco, we reach the bright stars Alwaid and Etanin (β and γ) of this constellation, and thence the stream passes to Lyra, where it seems to divide into two, one passing through Hercules, the other along Aquila, curving into the remarkable group Delphinus.

The streams here considered, include every conspicuous star in the heavens. But the question will at once suggest itself, whether we have not been following a merely fanciful scheme, whether all these apparent streams might not very well be supposed to result from mere accident. Now, from experiments I have made, I am inclined to believe that in any chance distribution of points over a surface, the chance against the occurrence of a single stream as marked as that which lies (in part) along the back of Grus, or as the curved stream of bright stars along Scorpio, is very great indeed. I am certain that the occurrence of *many* such streams is altogether improbable. And wherever one observes a tendency to stream-formation in objects apparently distributed wholly by chance, one is led to suspect, and thence often to detect the operation of law. I will take an illustration, very homely perhaps, but which will serve admirably to explain my meaning. In soapy water, left in a basin after washing, there will often be noticed a tendency to the formation of spiral whorls on the surface. In other cases there may be no definite spirality, but still a tendency to stream-formation. Now, in this case, it is easy to see that the curved bottom of the basin has assisted to generate streams in the water, either circulating in one direction, or opposing and modifying each other's effects, according to the accidental character of the disturbance given to the water in the process of washing.[1] Here, of course, there can be no doubt of the cause of the observed phenomena; and I believe that in every case in which even a single marked stream is seen in any congeries of spots or points, a little consideration will suggest a regulating cause to which the peculiarity may be referred.

It is hardly necessary to say that, if the stream-formation I have indicated is considered to be really referable to systematic distribution, the theory of a stratum of stars distributed with any approach to uniformity, either as respects magnitude or distance, must be abandoned. It seems to me to be also quite clear that the immense extent of the galaxy as compared with the distances of the 'lucid' stars from us, could no longer be maintained. On this last point we have other evidence, which I will briefly consider.

First, there is the evidence afforded by clusterings in the Milky Way. I will select one which is well known to every telescopist, namely, the magnificent cluster on the sword-hand of Perseus. No doubt can be entertained that this cluster belongs to the galactic system, that is, that it is not an *external* cluster: the evidence from the configuration of the spot and from the position it occupies, is conclusive on this point. Now, within this spot, which shows no stars to the naked eye, a telescope of moderate power reveals a multitude of brilliant stars, the brightest of which are of about the seventh magnitude. Around these there still appears a milky unresolved light. If a telescope of higher power be applied, more stars are seen, and around these there still remains a nebulous light. Increase power until the whole field blazes with almost unbearable light, yet still there remains an unresolved background. 'The illustrious Herschel,' says Professor Nichol, 'penetrated, on one occasion, into this spot, until he found himself among depths, whose light could not have reached him in much less than 4,000 years; no marvel that he withdrew from the pursuit, conceiving that such abysses must be endless.' It is precisely this view that I wish to controvert. And I think it is no difficult matter to show at least a probability against the supposition that the milky light in the spot is removed at a vast distance behind the stars of the seventh magnitude seen in the same field.

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[1] Sometimes a singular regularity of curvature is noticed, and a spiral is formed closely resembling in configuration some of the great spiral nebulæ, as drawn by Lord Rosse, so that one is tempted to see in the centrifugal tendency of the disturbed water, and the centripetal effects caused by reflection from the basin's surface, causes which may in some sense illustrate the laws operating in wider domains of space.

The supposition amounts, in fact, to the highly improbable view that we are looking here at a range of stars extending in a cylindrical stratum directly from the eye—a stratum whose section is so very minute in comparison with its breadth, that, whereas the whole field within which the spot is included is but small, the distance separating the nearest parts of the group from the farthest, is equivalent to the immense distance supposed to separate the sphere of seventh magnitude stars from the extreme limits of our galaxy. And the great improbability of this view is yet further increased, when it is observed that within this spot there is to be seen a very marked tendency to the formation of minor streams, around which the milky light seems to cling. It seems, therefore, wholly improbable that the cluster really has that indefinite longitudinal extension suggested by Professor Nichol. In fact, it becomes practically certain that the milky light comes from orbs really smaller than the seventh magnitude stars in the same field, and clustering round these stars in reality as well as in appearance.

The observations applied to this spot may be extended to all clusters of globular form; and where a cluster is not globular in form, but exhibits, on examination, either (1) any tendency within its bounds to stream-formation, or (2) a uniform increase in density as we proceed from any part of the circumference towards the centre, it appears wholly inconceivable that the apparent cluster is, not really a cluster, but a long range of stars extending to an enormous distance directly from the eye of the observer. When, in such a case, many stars of the higher magnitudes appear within the cluster, we seem compelled to admit the probability that they belong to it; and, in any case, we cannot assign to the farthest parts of the cluster a distance greatly exceeding (*proportionally*) that of the nearest parts.

Of a like character is the evidence afforded by narrow streams and necks within the galaxy itself. If we consider the convolutions over Scorpio, it will seem highly improbable that in each of these we see, not a real convolution or stream, but the edge of a *roll* of stars. For instance, if a spiral roll of paper be viewed from any point taken at random, the chances are thousands to one against its appearing as a spiral *curve*, and of course the chance against several such rolls so appearing is very much greater. The fact that we are assumed to be not very far from the supposed mean plane of the Milky Way would partly remove the difficulty here considered, if it were not that the thickness and extent of the stratum, as compared with the distances of the lucid stars, must necessarily be supposed very great, on the assumption of any approach to uniformity of distribution.

Evidence pointing the same way is afforded by circular apertures in the galaxy, or indeed by apertures of other forms, since a moment's inspection of figs. 1 and 2 will show the improbability of any tunnelling (so to speak) through the star stratum, being so situate as to be discernible from the centre. Another peculiarity of these cavities is also noticeable; whereas on the borders of every one there are many lucid stars, or in some cases two or three very bright stars, *within* the cavity there is a marked paucity of stars. This phenomenon seems to indicate a much closer connection between the brighter stars, and the milky light beyond, than is supposed on the stratum theory. One can hardly conceive the phenomenon to be wholly accidental.

There are some other points on which I would fain dwell, but space will not permit me. I may, perhaps, on another occasion, return to the consideration of the subject. For the present, I will merely note that there are peculiarities in the distribution of red double, and multiple stars, in the position in which temporary stars have made their appearance, and in the distribution of nebulæ, which seem very worthy of notice.

One point, however, immediately connected with my subject, remains to be mentioned. I have traced streams of stars *more* conspicuous than those forming the Milky Way. We have also evidence of streams of light yet more delicate and evanescent than the light of our galaxy. In Sir John Herschel's great work on the southern skies, he notes the frequent recurrence of 'an exceedingly delicate and uniform dotting, or *stippling* of the field of view by points of light too small to admit of any one being steadily or fully examined, and too numerous for counting, were it possible so to view them.' In thirty-seven places he detected this remarkable and significant phenomenon; a phenomenon so faint that he says, 'The idea of illusion has continually arisen subsequently;' an idea well befitting the modesty of the philosophic observer, but which those who appreciate Sir John Herschel's skill as an observer will be very unwilling to accept. As Professor Nichol remarks, 'It is enough to read from Herschel's note-book—"I feel satisfied the stippling is no illusion, for its dark mottling moves with the stars as I move the tube to and fro"—to feel convinced that the phenomenon is real.' Now, a remarkable fact connected with those observations is, that when Sir J. Herschel marked down in a star-chart the places in which he had detected this nebulous appearance, he found that, 'with the exception of *three*, which appeared outlying and disconnected, they formed several *distinct but continuous streams*.'

From the *Intellectual Observer* for August 1867.

STAR STREAMS AND STAR SPRAYS.

The stellar heavens present us with a problem of vast difficulty—the problem of determining the laws according to which those myriads of orbs which the unaided eyes can see, or which the telescope reveals, are distributed throughout space. We can determine the laws of stellar distribution so far as they relate to the imaginary concave of the heavens. We could form a globe upon which millions of stars might be indicated; or better, we might mark these millions of stars upon the interior surface of a hollow globe; and thus, so far as the apparent laws of stellar arrangement are concerned, we might actually render the eye cognisant of all which even the most powerful telescopes can reveal. But when this had been accomplished, we should have made but a short step towards the determination of the manner according to which the stars are distributed throughout space. We should have placed all the stars which the telescope reveals upon a spherical surface, whereas we know that they lie in reality on no such surface, but some at distances much vaster than the distances which separate us from others. We know that if we have to deal with a sphere of stars at all it is a sphere *full of* stars, and not a spherical surface *covered with* stars, that we have to consider. But, in truth, we know that the space containing all the stars revealed by powerful telescopes may not even approach the form of a sphere; and that within that space the stars may be distributed in the most irregular manner—here crowded most densely, there sparsely scattered, and throughout enormous regions mayhap altogether wanting; in some places arranged into clustering aggregations, in others in streams, and elsewhere in fantastic convolutions or reticulations; while, for aught that has yet been shown, the whole stellar region may be occupied more or less richly with a variety of forms of matter other than stars or suns, and even differing perhaps from any forms of matter with which we are acquainted.

Unfortunately we have no means of modifying the conditions of the problem presented to us by the stars. We must be content to analyse patiently the evidence by means of which alone the problem can be attacked. Whether when that has been done, or while it is being done, we shall be able to form clear and definite conclusions respecting the stellar system, need not at present concern us. It is at any rate certain that if the secret of the heavens is to be disclosed, it can only be as the result of systematic inquiries directed to the vast mass of information which has already been gathered together; and the doubts we may entertain as to the actual fruits of such inquiries ought not to deter us from making them, since that would be in effect to give up the problem of the star-depths altogether. To use the words of the astronomer to whom more than to any other, save one alone, we owe the power itself of making such inquiries, we must 'not be deterred from dwelling consecutively and closely on speculative views by any idea of their hopelessness which the objectors against "paper astronomy" may entertain, or by the real slenderness of the material threads out of which any connected theory of the universe (at present) has to be formed. *Hypotheses fingo* in this stage of our knowledge is quite as good a motto as Newton's *non fingo*, provided always they be not hypotheses as to modes of physical *action* for which experience gives no warrant.'[1]

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[1] From a letter written by Sir J. Herschel to me, in August 1869.

My purpose in the present paper is to pursue an inquiry (commenced by me some five years ago) into a certain peculiarity of the arrangement of objects within the star-depths, which appears to promise some insight into the real laws of stellar aggregation. I refer to the circumstance that there may be observed among the stars a tendency to arrangement in streams, of greater or less length, and more or less distinctly recognisable. I offer at present no explanation of the observed fact, but seek rather to convince the reader that this peculiarity has a real existence, and that it may be regarded as in fact a characteristic peculiarity of the stellar system. It must be mentioned, however, that the tendency to stream-formation among the stars is not to be regarded as universal. On the contrary, it is but a sign of a much more general law, according to which the stars are found to aggregate in certain regions and to be segregated from others, as though, in some long past era, forces had been at work which drew the star material towards certain regions of space, to the avoidance of others. And here again, I would invite attention to the fact that the study of these laws of stellar aggregation and segregation seems to afford the only means of attacking a problem of immense difficulty, to which the inquiring mind is naturally led by the partial success which has attended inquiries into the origin of our own solar system. We recognise so clearly within our solar system such motions and such laws of distribution as suggest a process of evolution, that the mind is led to inquire whether the motion of the stars and their arrangement throughout space may not indicate the action of a yet higher order of evolution. If the genesis of a solar system has been or is being revealed to us, may not the genesis of a galaxy be one day revealed in like manner?

But I merely point to such inquiries as these, in passing, by way of indicating the class of questions to which such phenomena as I am about to consider may eventually lead. Let us now turn to the discussion of those observed facts, simply, which seem to show that the stars in certain regions have been gathered into streams.

If we consider the stars according to their various orders of apparent magnitude, we are, in fact, treating the problem precisely as though the celestial vault were studied by means of telescopes progressively increasing in power. Nor need we, in so doing, make any assumption as to the real magnitudes of the stars. We know quite certainly that whatever telescopic power we use, or even when we study the heavens with the naked eye, we have to deal with objects lying at different distances, and that therefore we are exposed to the possibility of error arising from the fact that orbs which seem to be associated may in reality be in no way connected. But if we keep this possibility of error very carefully in view, and if we apply to the various cases which come under our notice such laws of reasoning as may best serve to eliminate such error, then, although we may not be sure that in all instances the error in question has been obviated, we shall yet have done much to obtain at least probable evidence respecting the laws of stellar distribution.

As an illustration of my meaning, I will take an instance belonging to the more general law of stellar arrangement, of which stream formation is but an instance. The reader is aware that the six stars which ordinary powers of sight recognise in the Pleiades are but a few among a very large number which are seemingly collected towards one particular region of the heavens in this place. Now, if we consider *only* two stars of the Pleiades, considerably unequal in magnitude, it must be regarded as not only possible, but (on *à priori* considerations) highly probable, that these two orbs lie at very different distances from the earth, and are not physically associated. But we are not free to extend this reasoning, which is admissible in the case of two stars, to the whole group of the Pleiades, and to argue that, because we have no means whatever of determining the actual distances of the orbs in that group, we are not at liberty to assume that they form a real clustering aggregation of stars. In so doing, we should undoubtedly be losing sight of evidence which absolutely demonstrates the clustering nature of the Pleiades. We have only to consider the mathematical probability that so many orbs would be gathered together within a certain portion of the heavens in the Pleiades, when the total number of stars between the same limits of magnitude is such and such, to see that we have not to do with an accidental phenomenon due merely to the apparent association of stars of many orders of distance in nearly the same direction, but with a real aggregation of stars into a definite cluster, surrounded on all sides by comparatively vacant regions. We know that William Mitchell, more than a hundred years ago, by simply considering the six brighter stars of the Pleiades, was able to show that the odds are about half a million to one against the association of these stars being apparent only.[1]

Now it is worthy of notice that, even among stars of the first three or four orders of magnitude, signs of aggregation are discernible, which appear too marked to be due to mere chance distribution. For instance, if we take an equal surface (*isographic*) chart of the northern heavens, showing all stars down to the fourth magnitude inclusive, we are struck by the singular vacancy lying where modern astronomers place the constellation of the Camelopard. Within an oval space, having Polaris and Castor as the ends of its longer diameter, Dubhe and δ Aurigæ as the ends of its shorter diameter, there are but three stars above the fifth magnitude, although this region extends over some fifty-eight degrees in length and about thirteen degrees in breadth.

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[1] Mitchell's paper, in vol. lvii. of the 'Philosophical Transactions,' anticipates in the clearest possible manner one of the general laws of stellar distribution which I have lately endeavoured to establish. The following passage, in particular, may be quoted in illustration:—'It has always been usual with astronomers to dispose the fixed stars into constellations; this has been done for the sake of remembering and distinguishing them, and therefore it has in general been done merely arbitrarily and with this view only; nature herself, however, seems to have distinguished the stars into groups. What I mean is, that from the apparent situation of the stars in the heavens, there is the highest probability that, either by the original act of the Creator, or in consequence of some general law (such, perhaps, as gravity), they are collected together in great numbers in some parts of space, whilst in others there are either few or none. The argument I make use of in order to prove this is of that kind which infers either design or some general law, from a general analogy, and the greatness of the odds against things having been in the present situation, if it was not owing to some such cause.'

But it is when we consider the stars down to the fifth magnitude inclusive that we first begin to recognise the existence of a marked tendency to stream-formation. It is among these stars, in fact, that we find those streams which the ancients recognised when they gave to certain star-groupings such names as Hydra, Draco, Serpens, the River Eridanus, and when they marked down among the constellation-pictures two streams from the water-can of Aquarius and a band connecting together the two fishes. The prolongations of some of these streams of lucid stars have been recognised by those modern astronomers who gave to certain southern star-groupings the names Hydrus, Reticulum, and the like.

Now, the chief question which has to be answered, in considering the evidences of stream-formation, is whether the streams are apparent only or real; and, in order to answer this question, we have to inquire what form or degree of streaminess (so to speak) might be expected among the 1,500 stars, down to the fifth magnitude inclusive, if these were really spread at random over the celestial sphere. In another of these essays I have indicated the means whereby

I have tested this matter, and the conclusion to which I have been led—*this* namely, that although among 1,500 or 2,000 points distributed at random over a surface of any kind, certain groups resembling streams might be recognised, such streams would not be nearly so well marked as the streams actually observed among the stars down to the fifth magnitude. But, on the other hand, it is not to be expected that the star streams actually recognised should be so exceedingly well marked and regular, or should be traceable over such great distances, that the reality of the stream-formation would be obvious at once. Had this been the case, indeed, the reasoning by which I have endeavoured to establish the reality of the phenomenon would not have been required. The first astronomers would have recognised the phenomenon as clearly as we can do. Therefore I do not consider the arguments which have been chiefly urged against these streams of lucid stars, regarded as having a real existence, as needing refutation. It has been urged that the streams can only be traced over such and such distances; that they can be carried this way or that, according to fancy, and so on. This, however, was to be expected; if it were otherwise, the reality of the streams would long since have been recognised: and apart from this, remembering that we are looking into the depths of space, and that, supposing star streams really to exist, we must see them foreshortened—in many instances projected on a background of stars less systematically distributed, and in other cases *mixed up* seemingly with other streams, either nearer or farther off—the wonder rather is that any well-marked *portion* of any stream should be recognisable, than that no stream should be traceable over very large areas on the heavens, and still less from its beginning to its end. That the reader may form his own opinion as to the reality of the streams traceable among stars down to the fifth magnitude, I give the case of a star-group which is certainly not the most remarkable for streaminess, but chances to be more convenient for the purposes of illustration than most others. Fig. 3 presents the stars forming the connecting band of Pisces. The bright star in the lower left-hand corner is the knot of the band, one part of the band being formed by the curved stream of stars passing to the lower right-hand corner, the other by the curved stream passing, with an inflection near the double star in the figure, towards the upper right-hand corner. In this figure the fact that certain sets of stars lie on certain curved lines is of slight significance, for assuredly in any chance distribution of stars the like would be found; the fact which is really significant is the paucity of stars on either side of the curved streams. We have certain lines along which the stars are plentifully strewn, while the adjacent

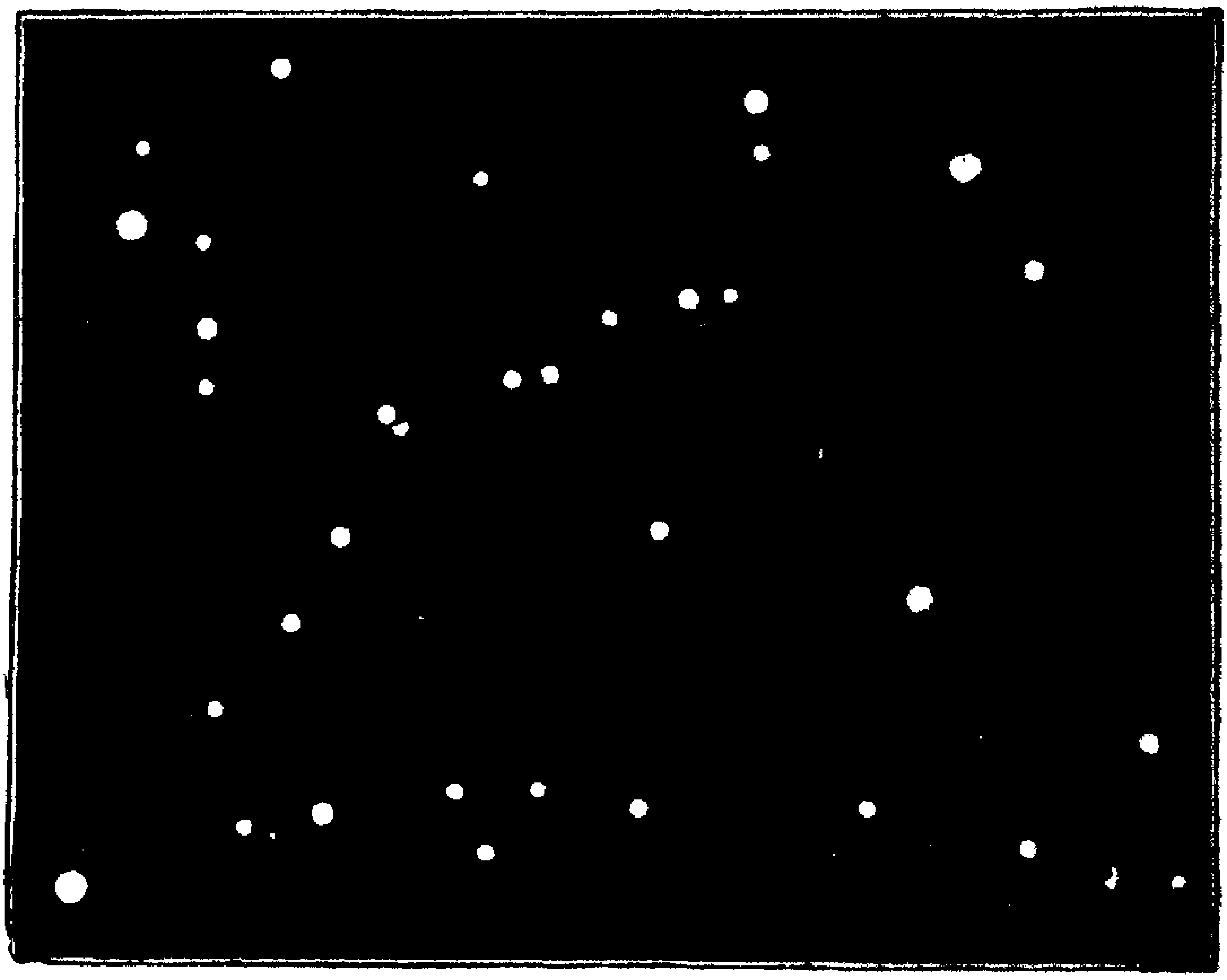

FIG. 3.—Showing the stars which form the connecting band of Pisces, &c.

spaces are relatively vacant. This feature, recognisable not only in this case, but in others, and even more markedly in several instances, is one which cannot reasonably be ascribed to mere coincidence. Let it be noted, moreover, that whatever significance we attach to it, when considering the stars of the first five orders of magnitude, must be enhanced if, as we proceed, we recognise a similar feature (on a different scale, however) among stars of lower orders of magnitude. Throughout this paper, I am not presenting a series of considerations so related one with the other that the failure of one destroys the validity of my reasoning; I am dealing with arguments which are independent of each other, though severally adding to each other's strength. If some of them fail, my case is only *pro tanto* weakened; it is not by any means destroyed.

Before leaving fig. 3, however, I would invite special attention to the manner in which the two star streams are conjoined. We see these streams converging upon a single star brighter than those which form the streams themselves; and we may also trace, not indistinctly, a certain general equality of distribution among the stars of the two streams. The former feature is, however, the only one I care at present to dwell upon; and it is to this particular arrangement of streams—two or more (but usually two) proceeding from a single star—or of branches proceeding, as it were, from a single stem, that I have given the title of star sprays. In searching among the star-depths revealed by telescopes of considerable power, many cases may be noticed in which such star sprays exhibit a singular uniformity of structure. The stars of the leading magnitudes are too few in number to afford many well-marked instances. I may note, however, the arrangement of the stars in Coma Berenices as one illustration of this sort; the stars γ, 14 and 13, forming the stalk of the spray. Another illustration may be recognised in the stars forming the poop of Argo and the hind-quarters of Canis Major, or (to use a more satisfactory way of indicating the orbs I refer to) the streams of stars converging on ξ and ρ Argûs, from ε Canis Majoris and from π Argûs. At ε Canis Majoris there is another subdivision; one stream of stars passing to κ Columbæ, the other over *u* and *x* Puppis to ν Argûs. The streams from the water-can of Aquarius form a more extensive, but perhaps less satisfactory, illustration of the same peculiarity.

I need give the less attention to those cases of stream-formation which may be recognised among the stars of the first six orders inclusive, because I have already discussed the relations among the lucid stars, in the preceding essay and in 'Other Worlds.' Of the peculiarities of *distribution* recognisable among the stars there dealt with, I may say with confidence that it is wholly impossible to regard them as accidental; they indicate beyond all possibility of question the existence of some real cause which has led to a drifting of the stars towards certain regions. As regards such peculiarities of arrangement as would fall more particularly under the head of my present subject, I think it is almost equally impossible to feel any doubt. If some of the streams and reticulations which can be recognised in the isographic charts forming Plates I., II. and VI. be due to chance distribution, the coincidence is very much more remarkable than the theory of star streams which I am at present advocating. It is truer to say, however, that the laws of probability as at present understood will not permit us to regard such singular configurations as accidental.

It would be desirable that we should have equal-surface charts of the heavens to include stars down to the seventh, eighth, and ninth magnitudes severally; because it is only by thus considering the separate stages of space-penetration that we can obtain complete recognition of the laws of stellar distribution throughout space. We owe, I think, to the elder Struve the first recognition of the importance of such graduated advances within the star-depths; though he dwelt rather on the importance of star-gauging (and that, also, according to averages) than on the value of star-charts capable of revealing to the eye the statistics of stellar distribution. It will not be difficult to construct charts including stars down to the seventh, and eighth, and ninth orders of magnitude; because as soon as the complete survey of the heavens has been effected after the plan already extended by Argelander to the northern hemisphere, the charts forming the survey, if carefully drawn,[1] will enable us to construct charts of complete hemispheres including stars down to the seventh, eighth, and ninth magnitudes severally inclusive.

At present, however, for want of such intermediate charts (so to speak) I pass from my equal-surface projection of all the stars down to the sixth magnitude inclusive, to an equal-surface projection which I have just completed, in which all stars in Argelander's series of forty northern maps have been marked in with careful reference as well to their arrangement as to their magnitude. In these forty charts, as many of my readers are doubtless aware, Argelander has included all stars down to magnitude nine and a-half, within ninety-two degrees of north polar distance—the two degrees south of the equator being added in order to facilitate the comparison of the northern atlas with charts forming the southern survey, one day to be completed (it may be hoped) at southern stations. In all there are 324,198 stars. All these I have carefully copied in, upon a circular chart two feet in diameter, isographically divided (in pencil) by radial lines and circles, into spaces extending one degree in declination and one degree in right ascension for sixty degrees to the north of the equator, the nominal extension in R.A. being correspondingly increased with proximity to the pole. In fact, all the spaces in Argelander's series of charts (some 26,400 in all) were represented in pencil in my projection, before a single star was charted in. Then the stars were carefully copied in, space by space, from Argelander's atlas; at such a rate (on the average), that the whole work of charting occupied me about 400 hours.[1] I do not think that the labour was thrown away, when it is remembered that, as a result, the statistical distribution of all the stars down to the 11th magnitude of Sir J. Herschel's scale was presented to the eye. The gauges of the Herschels had included in all about 160,000 stars, and Struve, in the elaborate series of inquiries on which he founded the theories propounded in his 'Études d'Astronomie stellaire,' dealt with about 32,000 stars; but the labours of Argelander enabled me not merely to count, but to delineate, 324,198 stars—not merely to draw inferences from statistical enumeration as to the real laws of stellar distribution, but to exhibit those laws of distribution to the eye.

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[1] It is important that the size of the discs used to indicate the several magnitudes should remain unchanged during the whole process of engraving, and also that the several charts forming the series should be printed with exactly the same degree of *fulness*. In Argelander's splendid series of forty charts, in which all the northern stars down to the magnitude intermediate between the ninth and tenth are included, slight changes have taken place during the progress of the work, which create some degree of doubt as to the orders to which the stars belong in some of the charts.

The first and most important conclusion deduced from this process of charting relates to the Milky Way; but it will be well to defer the consideration of that conclusion until I come to speak of the Milky Way as itself a vast conglomeration of star streams and star sprays.

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[1] Argelander and his assistants were engaged no less than seven years and one month in completing their magnificent contribution to uranography. The rate at which I copied in the stars was such as to enable me to copy carefully within each space on my projection the stars shown in the corresponding space in the large atlas. It is worthy of notice that a single second of extra time (on the average) per star would have caused an addition of ninety hours', or say ten days', work. The time actually employed on the average was slightly less than four and a-half seconds per star.

But another conclusion, not obviously deducible from the chart itself, or its photographic reductions, was forced upon me in a very marked manner as the work of charting proceeded. Again and again I had occasion to notice the tendency of the stars to associate themselves into streams and sprays, the star sprays being ordinarily of the form illustrated in fig. 3. It would be quite impossible for me to convey by means of pictures any adequate idea of the persistence with which these peculiarities of structure were renewed, especially in certain parts of the chart. Indeed, it would be absolutely necessary to include a much larger portion of the heavens than could here be conveniently pictured, to show the most significant feature of all, the manner, namely, in which the sprays are divided and sub-divided.

Now the point to be chiefly noticed here is, that so far as *à priori* considerations are concerned, one would be led to expect that no very marked signs of stream-formation would be shown among stars down nearly to the tenth order of magnitude. For the further we increase our range of vision, the more likely must we be, it would seem, to obtain a view in which the actual relations of the stars are confused by seeming combinations, due to the accidental agreement, in general direction, of star-groups at very different orders of distance. For either there is or there is not a general uniformity of star-magnitude (within certain limits). If there is such uniformity, so that the fainter stars are in reality as large (on the average) as others, but more distant, then it is certain that amongst the stars dealt with in Argelander's charts there are in all directions stars at very different distances; stars therefore not in reality associated, but which yet, being seen in nearly the same direction, are brought into seeming juxtaposition. If, on the other hand, there is not among the stars, even in a general sense, any approach to uniformity of magnitude, then we may be even more completely deceived; for certain near stars which are really very small (relatively) may be brought, not merely into seeming juxtaposition with more distant stars, but even to a seeming equality of magnitude.

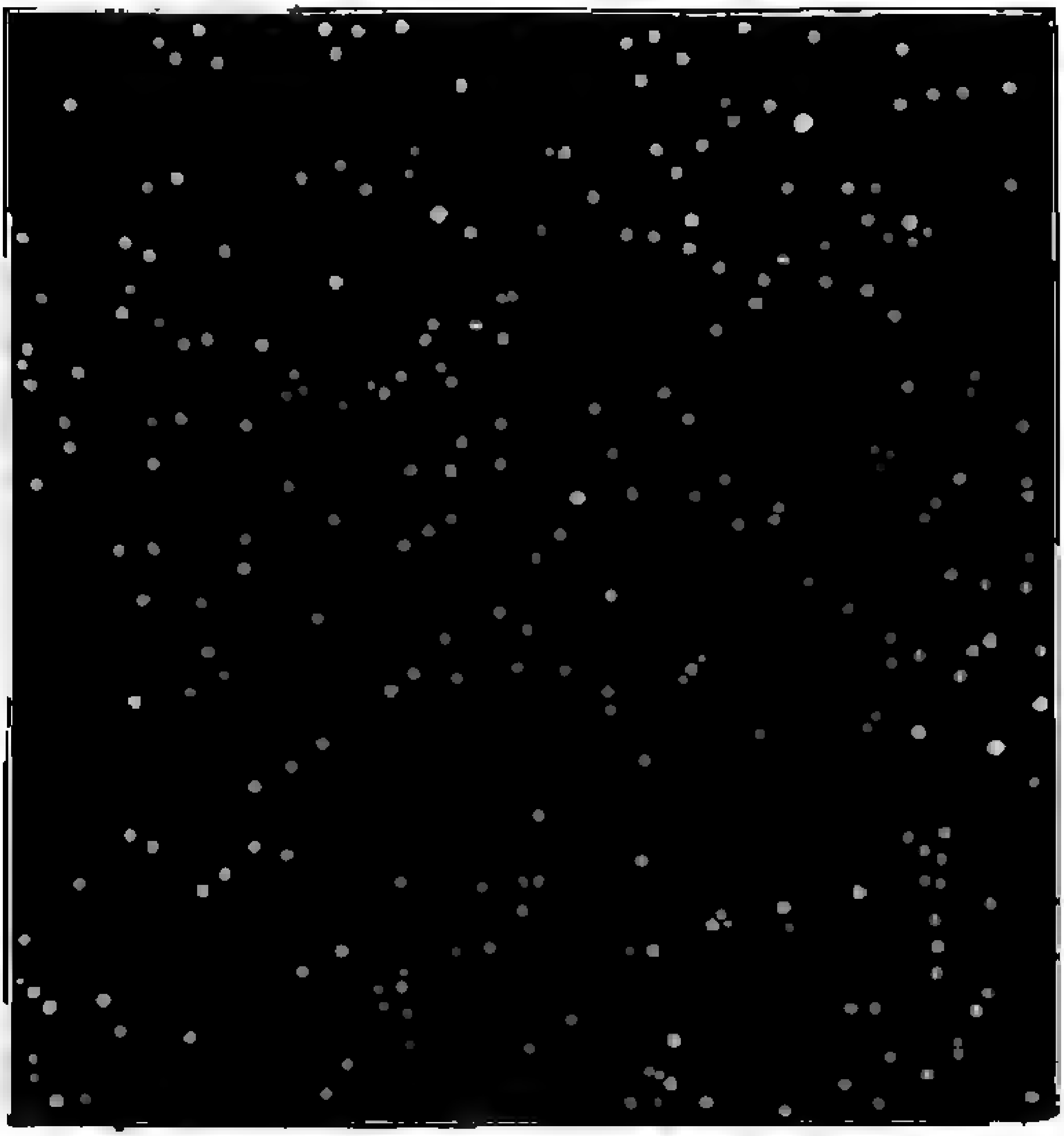

Fig. 4.—A portion of one of Argelander's charts, the centre of the space here shown being nearly in 32½° N. Dec., and 10h. 28m. R.A.

This being remembered, it was to be expected that the distribution of the stars included in Argelander's charts would correspond much more closely to a real chance distribution of so many points over a hemisphere, than where we considered only a comparatively small number of stars belonging to the leading orders of magnitude. Moreover, even assuming the point which I am now endeavouring to prove, viz. that the stars are in many cases arranged into the form of streams and sprays, it would yet seem highly probable that all signs of these streams and sprays would be obliterated, simply because streams and sprays of stars would probably lie at very different distances in all directions, and the configuration of the nearer streams would be blended with and confuse the configuration of the more distant.

Figs. 4 and 5 will serve to illustrate my meaning. It is not difficult to recognise in both these figures the existence of star streams and star sprays too well marked to be regarded as due to accident; but yet we are led to suspect that the streams here seen lie at different distances, and that much more clearly marked streams would be recognised if we could cut off with a veil all the stars beyond a certain distance, and obliterate altogether certain of the nearer stars. I would, however, recommend such of my readers as possess Argelander's chart to study the region around the two spaces pictured in figs. 4 and 5; when I think the conviction will be forced upon them that there is a much closer connection between the several branches of stars seen in those regions than one would have been disposed to expect among the orders of stars Argelander has included in his charts.

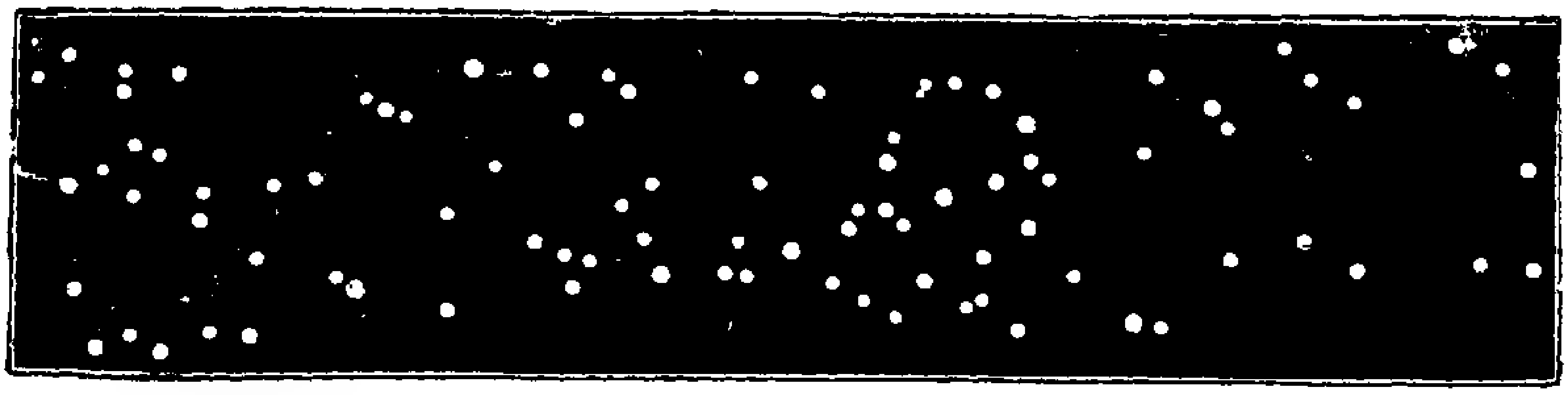

FIG. 5.—A portion of one of Argelander's charts, the centre being in about $26\frac{1}{2}°$ N. Dec., and 22h. 8m. R.A.

Many cases occur, however, in which two streams lying at different distances appear to cross each other in the chart; and it is a somewhat noteworthy circumstance, that the disposition of five nearly equal stars in the form of a cross, thus .:. which is very seldom met with (compared with other simple configurations) in the complete series of charts, is commonly to be noticed where two well-marked streams cross each other.

The arrangement of the stars in the large chart, as respects aggregation in certain regions and segregation from others, is sufficiently remarkable; but I have not space to dwell at length here on peculiarities of that description. Some of these peculiarities are associated with the configuration of the galactic stream of stars, presently to be briefly referred to. One, however, is so remarkable that I cannot refrain from here calling special attention to it. The Milky Way region or zone is shown in the chart to be exceptionally rich in stars (as W. Struve judged from statistical considerations); but instead of that gradual tendency to aggregation towards the galactic zone which Struve supposed to prevail, there is in many places a sudden change in the density of distribution, spaces close by the galaxy being relatively poor. But in no instance is this peculiarity so remarkably exemplified as in the part of the Milky Way near the horns of Taurus (see Plate XI.). Here we have on one side the rich fields of the Hyades and the Pleiades, and on the other rich galactic fields—properly so called; but between these two rich regions we have absolutely the poorest region in the whole of the northern heavens.[1]

Extending next our range of view so as to reach the stars down to the thirteenth magnitude inclusive, we have indeed less complete surveys to consider, but yet the evidence we obtain is sufficiently distinct. The zodiacal zone has been closely surveyed by Chacornac, Hind, and others, with the object of so mapping down even the fainter stars, that the asteroids which traverse this region may be the more readily recognised. In the maps thus constructed we find star streams and star sprays as well marked as in Argelander's chart. As the same general considerations apply in this case, it will be sufficient for me to invite attention to figs. 6 and 7; but I would recommend the student who may possess Chacornac's charts to study carefully the regions which surround the two spaces pictured in these figures.[1]

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[1] This peculiarity did not escape the attention of Argelander, who says:—Die absolut ärmste Gegend findet sich aber sonderbarweise nicht gar weit von der Milchstrasse entfernt, an den Hörnern des Stiers.'

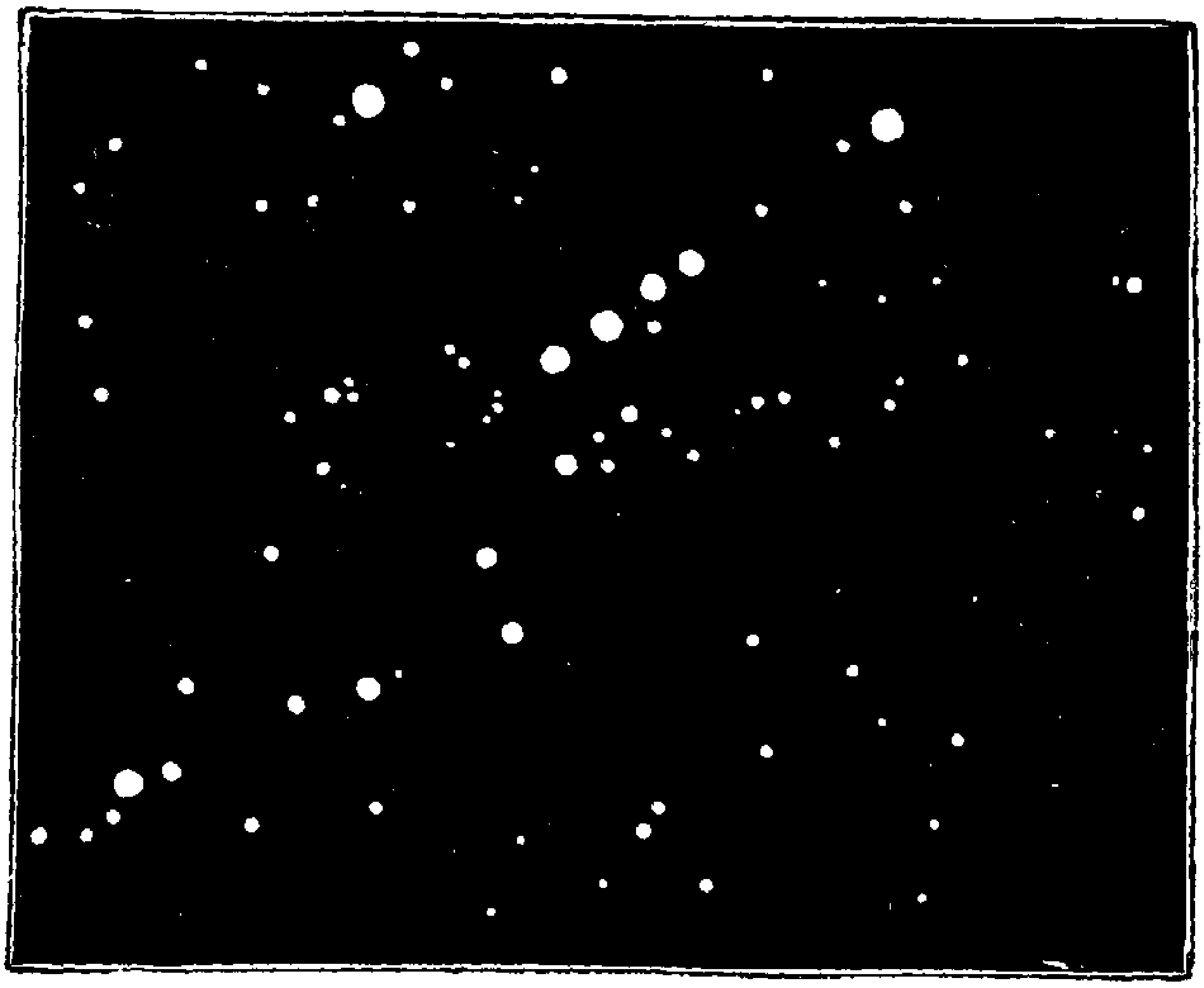

FIG. 6.—A portion of one of Chacornac's ecliptic charts, the centre of the space here shown being in 1° N. Dec., and 23h. 43m. R.A.

If we pass on towards yet more remote depths, we still find well-marked signs among the stars of a tendency to form streams and sprays. Sir John Herschel has pictured some very singular specimens of such streams, as seen in his eighteen-inch reflector during his survey of the southern heavens; and doubtless, could the fields surveyed by the elder and younger Herschel be presented in maps, so that several adjacent fields could be seen at a single view, many other instances would be added to the list.

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[1] There is an interesting quotation at page 267 of Webb's 'Celestial Objects,' in which Fr. Secchi describes the astonishment with which, when studying certain galactic regions, he saw spirals and curves of stars so regularly disposed as to preclude all possibility that chance distribution was in question.

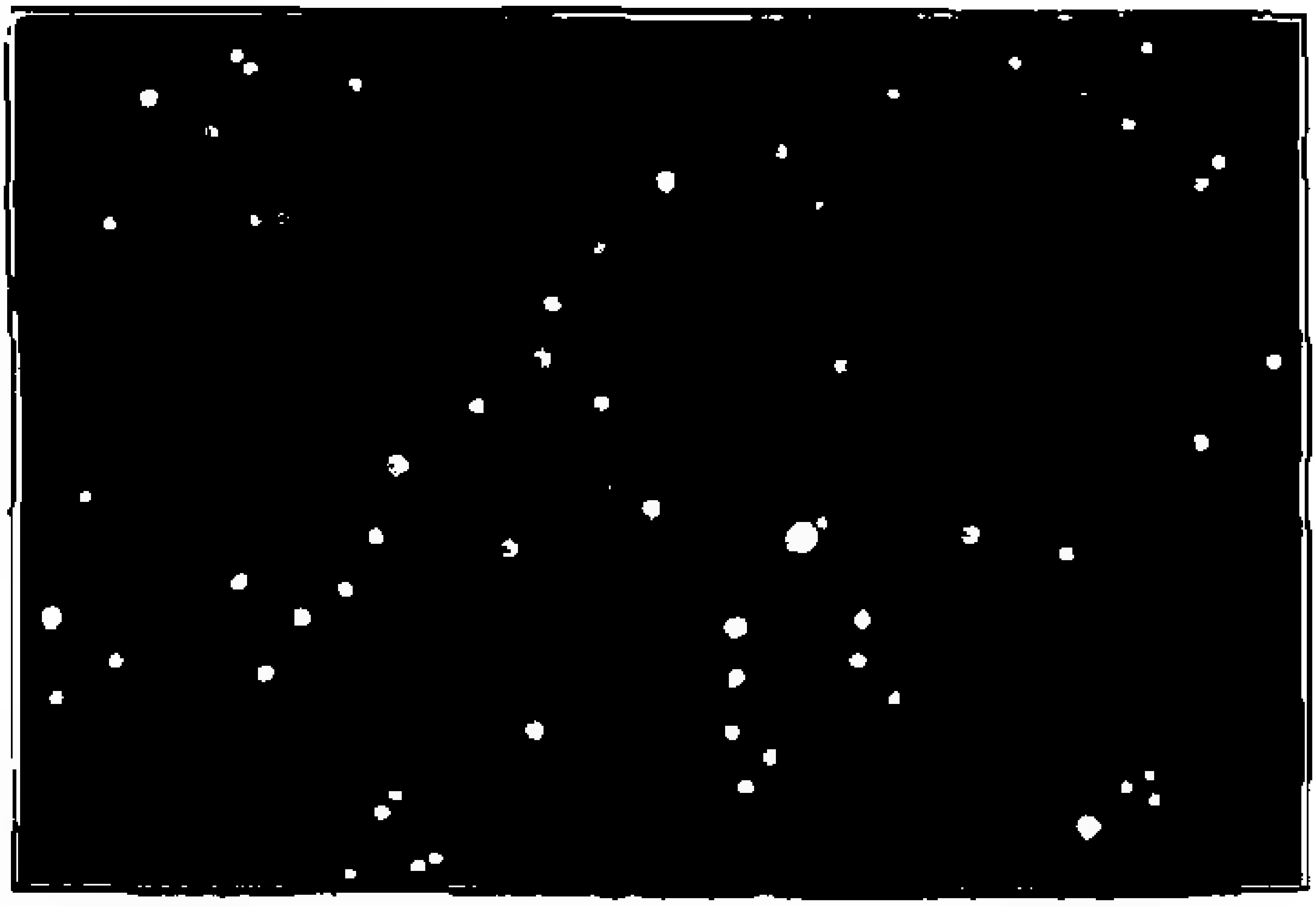


FIG. 7.—A portion of one of Chacornac's ecliptic charts, the centre being in 4½° S., and 23h. 26m. R.A.

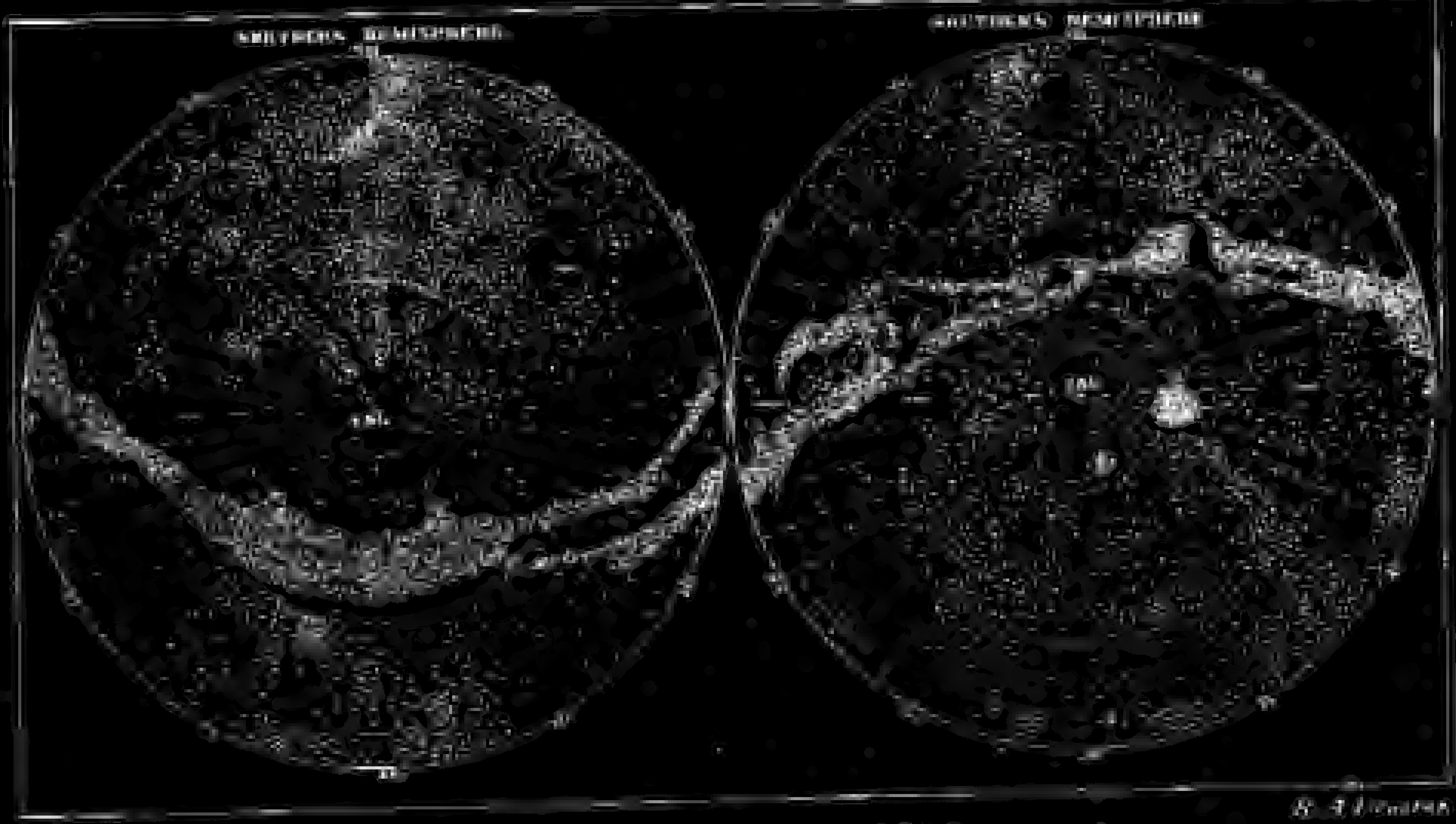
It is scarcely necessary to add that the largest telescope ever made by man—the great Parsonstown reflector—has revealed even in the intricate constitution of the nebulæ the existence of streams and sprays, sometimes spiral, sometimes but slightly curved, sometimes disposed with singular regularity, at others extending in irregularly shaped branches, growing gradually fainter and fainter, until they are at length lost altogether either by diffusion or through extreme faintness.

But it remains to be considered that we have strong evidence in favour of the view that the Milky Way itself is but a stream, or rather a congeries of streams of stars. The evidence on which Sir William Herschel rested his theory that the galaxy is of the shape of a cloven flat disc, was abandoned by himself during the later years of his career as an observer; and he recognised clearly that some of those rich nodules of the Milky Way which can be seen in the northern heavens are real aggregations of stars (not vast depths along which the stars are arrayed as in a sort of procession), and that such aggregations approach in figure to the spherical form. In the southern heavens Sir John Herschel recognised galactic regions to which Sir William Herschel's later mode of reasoning could be applied even more convincingly. Now precisely the same reasoning by which Sir W. Herschel was led to regard the rich clustering regions of the Milky Way in Cygnus as spherical in form seems to show that the well-marked portions of the galactic stream are really stream-shaped. And this view of the galaxy, which might seem to agree ill with the usual account of this marvellous band of stars, accords excellently with the description given by Sir John Herschel and others who have most carefully studied the galaxy. More especially is it suggested by the aspect of the Milky Way in the southern heavens. For there the continuity of the zone, on which so much stress had been laid, is shown to be interrupted by a broad dark rift, a feature wholly inexplicable on the theory that the Milky Way is shaped like a cloven flat disc. And over the whole region from Argo, over the feet of the Centaur, to Sagittarius and Scorpio, the Milky Way as pictured by Sir John Herschel presents an appearance far more closely according with the theory that the Milky Way in this region forms a gigantic spray of stars than with any other that has been propounded. In the northern heavens, the faintness of the Milky Way causes it to appear more uniform in structure; but even in the northern heavens, as has been well pointed out by Professor Nichol, it is only on the most cursory examination, or when the Milky Way is studied under unfavourable circumstances, that it can be regarded as a simple zone (see Plate VI). But it is well worthy of notice that in my chart of 324,198 stars, the Milky Way reveals itself (through the mere aggregation of stars down to the 9⅓th order) as a congeries of streams, with branching extensions, of which only the commencement can be recognised as more or less marked projections, in the best pictures of the northern parts of the Milky Way (see Plates XI. and XII.).

It remains to be noticed, in conclusion, that the nebular system also shows the most marked tendency to stream-formation when isographically charted, as in Plates III., IV., and V., and the tendency to stream formation is more especially to be noticed among the southern nebulæ. It is worthy of remark that, whereas the southern nebular streams converge upon the Magellanic Clouds, the northern nebular streams seem to extend towards the outlying streams of the Milky Way, as it appears in my chart of 324,198 stars. The evidence of a real association between stars and nebulæ is singularly strengthened by these peculiarities of arrangement.

From the *Popular Science Review* for October 1871.

PLATE II



Isographic Projection.

R. A. Proctor.

I. DISTRIBUTION OF THE NEBULÆ.

The Northern & Southern Nebulæ

NOTES ON NEBULÆ.

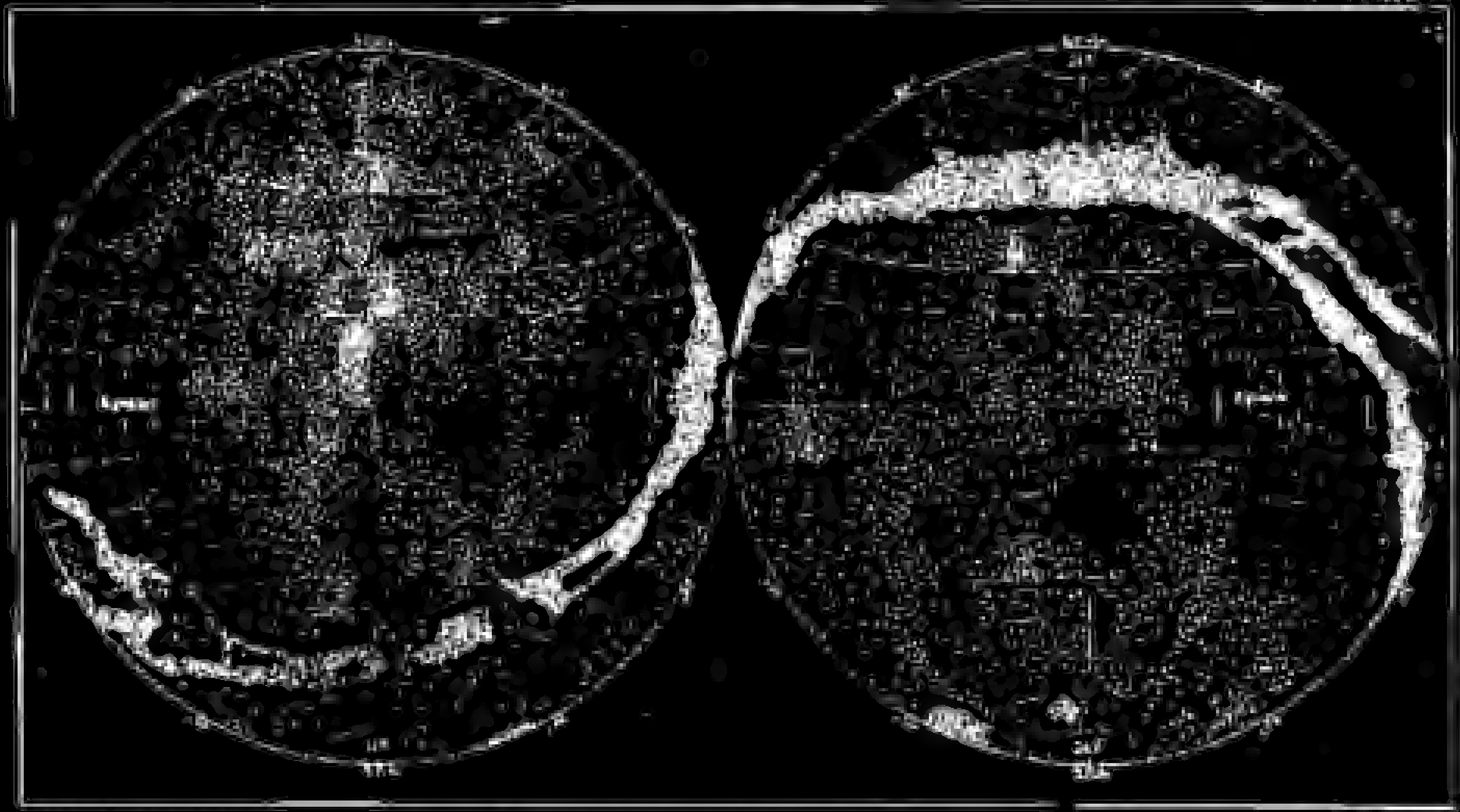
Including both hemispheres, there are visible to the naked eye, under the most favourable circumstances, about 5,000, or at the most 6,000, fixed stars. If these and the fixed stars visible in the most powerful telescopes were to disappear, and the eye to acquire the light-gathering power of Herschel's 20-feet reflector, there would be seen scattered over the vault of heaven—instead of the fixed stars now visible—about as many nebulæ—'feebly shining, cloud-like patches, often of strange and fantastic forms.'

The faint illumination of nebulæ, the close crowding of stars in many that are resolvable, and other circumstances, have appeared to confirm the view that these objects shine from distances far beyond those of the farthest fixed stars. It has been considered that nebulæ are (for the most part, at least) aggregations of suns ('island-universes,' as a German philosopher has expressively termed them), forming galaxies similar to our own 'Milky Way,' and reduced, notwithstanding the immensity of their dimensions, by a yet greater immensity of distance, to the appearance of faint specks of light, which the slightest haze in our skies is sufficient to conceal from us. The important discoveries effected by Dr. Huggins, which have shown that many nebulæ are gaseous in composition—nineteen out of sixty observed presenting the spectrum of bright lines separated by dark spaces which indicates the gaseity of the source of light—have modified these views. Without giving absolute support to the speculations of Sir W. Herschel, respecting the condition and changes of condition of nebulæ, Huggins's discoveries show that the views on which Herschel founded his speculations were well grounded. The distinctions Herschel drew between the different classes of nebulæ have been confirmed, so far as spectrum analysis has yet proceeded. Every planetary nebula yet observed, for instance, has been proved to be gaseous. The process of generalization, which had been commenced by many eminent astronomers, and in which scarcely any distinctions but those depending on the resolvability of nebulæ were recognised, has been abruptly checked. It is to be noticed, however, that many German astronomers, and some, at least, of our most distinguished English observers (amongst others, the late Admiral Smyth) had looked with more than doubt on the view that, with sufficient optical power, all nebulæ are resolvable into stars.

It is clear that Huggins's discoveries affect, not merely the views we must form of the constitution of nebulæ, but our ideas of the distances at which these objects are placed, and, therefore, of their true magnitudes. It is with such considerations as are suggested by modern discoveries, and with others for which the overthrow of opinions lately thought well established seems to leave room, that I wish now to deal. Anything like a discussion of the *general* subject of nebulæ, a sketch, however brief, of the history of their discovery, or an account of particular nebulæ (save in illustration of the considerations I have to deal with), would be wholly out of place in this essay—occupying, as it does, the position of a simple preliminary dissertation, indicating results on which sequent discussions are to be based.

The first point I shall dwell upon is the distribution of nebulæ over the heavens. In the present state of our know-

PLATE IV



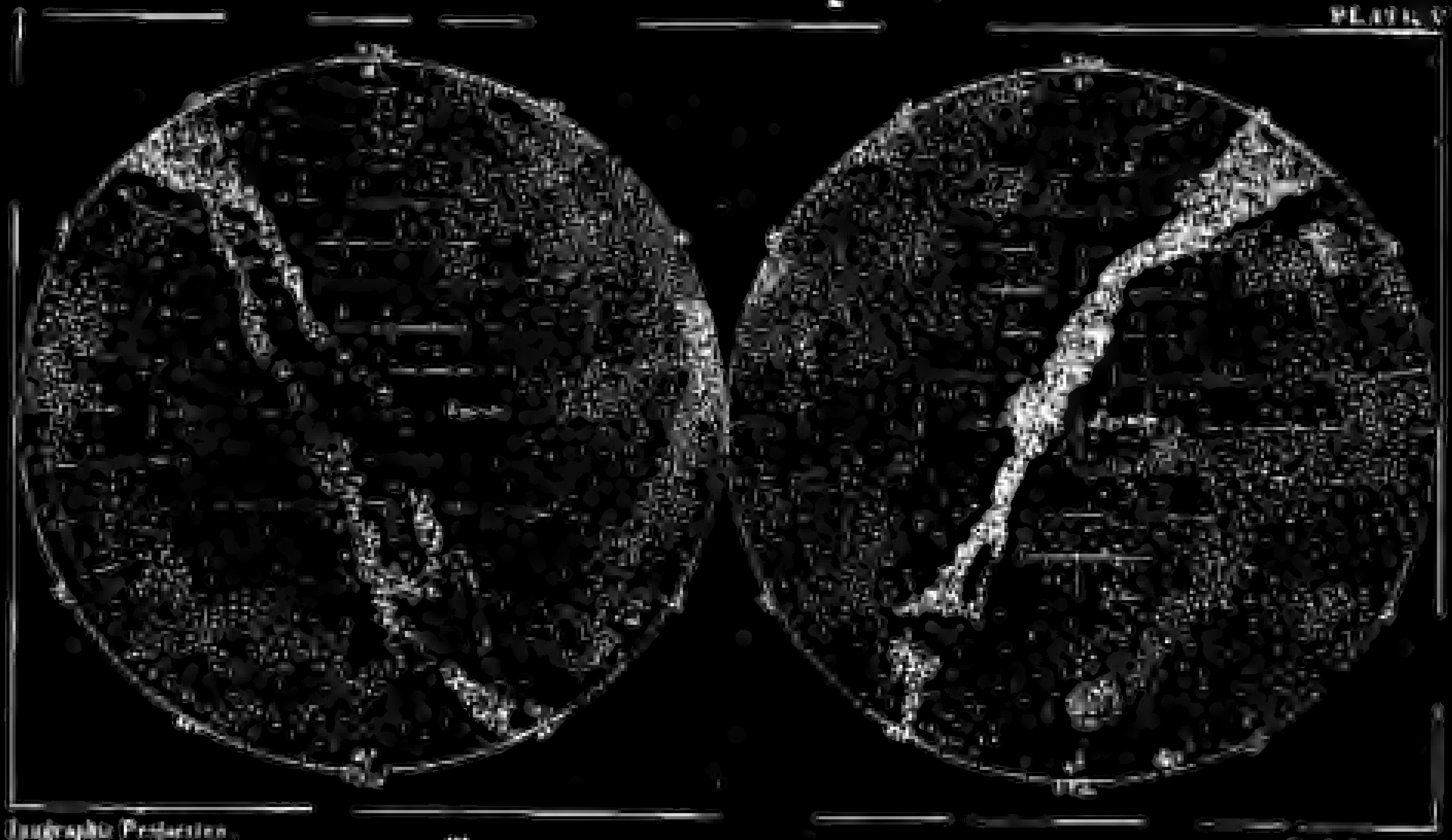
Stereographic Projection.

R. A. Proctor

II. DISTRIBUTION of the NEBULÆ.

The Great Nebular Groups

PLATE V.



III. DISTRIBUTION of the NEBULÆ.
The Zone of Few Nebulæ.

ledge, it would not be convenient to particularise classes of nebulæ in considering this point—though, as we shall presently see, there are one or two circumstances in this connection which cannot be wholly neglected.

Sir John Herschel, in order to form a clear conception of the distribution of nebulæ over the celestial sphere, adopted a plan similar to that illustrated in Plate III. But he represented the number of nebulæ falling in each space by numbers—a method which does not serve to present very clearly the distribution of nebulæ. In Plates III., IV., and V. I have indicated the number of nebulæ falling in each space by dotting. Let us consider what evidence the maps seem to present of orderly distribution.

In the northern heavens there will be observed a very decided clustering in the region between 11h. and 14h. of R.A. This is the nebular region in Virgo,[1] extending over Coma Berenices, and the tail of Leo, curving (to the right in our figure) over Canes Venatici, thence (to the left) over the tail and hind quarters of Ursa Major, to within about 12° of the pole near the tail of Draco. The borders of this stream or cluster of nebulæ extend dispersedly over the two Leones, Cancer, Gemini, Lynx, and Ursa Major on one side, and over Bootes and Corona on the other. As there seems to be a decided break in the stream—or rather, perhaps, as the stream decidedly comes to an end near the pole—we must return to the point from which we commenced, and thence follow the stream in the contrary direction. The continuation of the stream presently divides into two, the right-hand stream passing over the left hand of Virgo, the tail of Hydra, and nearly the whole extent of Centaurus, to Crux and Musca; the left-hand stream passing over Crater, to Antlia, and the mast and sails of Argo. The gap which bounds the northern group seems continued, but not in quite so marked a manner, by the space comparatively clear of nebulæ which runs round the right-hand stream (of the two just described) across the pole. Returning to the northern heavens we notice a less distinctly marked grouping over part of Perseus and Andromeda, passing (to the right) over the square in Pegasus to the southernmost of Pisces, and (to the left) over the band in Pisces, across Cetus, Eridanus, and Dorado, in a distinctly-marked stream leading to the Nubecula Major. The right-hand stream, which we had followed as far as Pisces, seems to have a continuation towards the Nubecula Minor, and also to throw out a convolution over the tail of Piscis Australis, over Indus et Pavo, towards Apus and Musca.

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[1] The readiest way of ascertaining where the constellations lie is to examine Plate VIII., in which the nebulæ and the lucid stars are included.

There remains to be noticed a clustering of nebulæ towards the portion of the Milky Way occupied by Scorpio. Sir John Herschel considers that many of these nebulæ belong to the Milky Way, as they are wanting in the gap between the two branches of the galaxy in this neighbourhood.

The first inquiry which suggests itself, on a review of the distribution of nebulæ, is the question, whether there is any indication of a connection between nebulæ and fixed stars? The theory that nebulæ are galaxies similar to our own Milky Way would, of course, require that we should dissociate nebulæ from any connection with our galaxy, save a relation corresponding to that which holds between the fixed stars and the sun. And further, although it would not be impossible that a tendency to systematic arrangement should be apparent among the nebulæ, yet the distances separating nebula from nebula would (on this theory) be so vast, compared with the distances separating star from star, or even with the dimensions of our galaxy, that it would clearly be very improbable that such arrangement should be discernible by terrestrial astronomers. As, however, the theory is very generally held, I shall present, as I proceed, some considerations which seem opposed to it. We seem, rather, to have evidence that most of the nebulæ—if not all of them—are much nearer to us than has been commonly maintained. I must observe, however, that my object is neither to establish nor to overthrow theories, but merely to note some of the salient points of the subject in hand, which have, as it seems to me, been unduly neglected in our works on Popular Astronomy.

Sir J. Herschel, after presenting the chief features of the distribution of nebulæ, remarks that 'the general conclusion which may be drawn from the survey is, that the nebulous system is distinct from the sidereal, though involving, and, perhaps, to a certain extent, intermixed with the latter.' Setting aside the 'band of clusters' near Scorpio, already referred to, he considers that the distribution of nebulæ is explained by considering the nebular region in Virgo to be the central condensation of a spherical (roughly speaking) cluster of nebulæ; that our system lies outside the denser part of the cluster, but is 'involved within its outlying members,' or 'forms an element of some one of its protuberances or branches, of which the individuals are the sporadic nebulæ confusedly scattered over the general surface of the heavens, and of which the prolongation towards the constellation Pisces may give rise to the apparently denser grouping of the nebulæ in that region.'

To me these views appear wholly untenable, and I feel the less diffidence in expressing dissent from so high an authority as Sir J. Herschel, because he has elsewhere expressed opinions which show that he held the above theory by but a light grasp. If the dense mass of a cluster of nebulæ lay in one direction, and but outlying branches in the other, the number of nebulæ seen in the former direction should overwhelmingly exceed the number seen in the other; whereas, at the outside, the proportion in the actual case is but as two to one, and even this excess is no doubt partly due to the fact that the southern hemisphere has been less thoroughly searched than the northern. And neither this view, nor the other solution suggested by Herschel, explains the existence of a zone of the celestial hemisphere in which nebulæ are markedly wanting. If our system were placed within a *narrow* branch, or protuberance, of a nebular system, the cluster in Cetus should be smaller than it is; if, on the other hand, the branch were of such dimensions as our conception of the distances of nebulæ would lead us to expect, there would not be wanting nebulæ on every side of us. These, however, are not the objections on which I would mainly dwell. I shall be able to show, I think, that the appearance of nebulæ, and the relations they present to the fixed stars, are opposed to the view that they form an independent system, of which our galaxy is but a member.

Singularly enough, Sir J. Herschel, immediately after exhibiting the views presented above, quotes (and as if in corroboration) an opinion of his father's, which is very strongly opposed to those views. He says, 'It must not be left out of consideration, and has been distinctly remarked by Sir W. Herschel, as an element of whatever speculation a closer attention to this subject, and a more perfect classification of nebular objects may lead us to indulge in, that the most condensed portion, and what may fairly be regarded as the principal nucleus of the region of Virgo, is situated almost precisely in one pole of the Milky Way.' Why, it may be asked, should the nebulæ, supposing they really form a system of which our galaxy is but a member, show any tendency to aggregation about either pole of the Milky Way? Assuming our galaxy to be but a member of

THE STARS to 6th M. [illegible] ade in the B.A. CATALOGUE

(On an equal [illegible] Projection)

DRAWN BY RICHARD A. PROCTOR

The Northern Milky Way from [illegible]eis, the Southern from Sir J. Herschel a supposed nebulous system, to *expect* any relation of the kind described would be as unreasonable as it would be for an astronomer on Saturn to look for a connection between the arrangement of the fixed stars, and the apparent position of the Saturnian rings upon the celestial firmament. And therefore, if such an arrangement is actually observed, it must be accepted either as accidental—in which case it is clearly not necessary to consider it in forming a theory of the nebulæ; or else we must view the arrangement as an evidence of a closer connection between the nebular system and the Milky Way than is commonly assumed to exist.

Tracing the group or stream whose condensed region is in Virgo from its commencement near the tail of Draco, we find that it crosses a region rich in small stars and double stars. Coma Berenices, a constellation which may almost be termed a star-cluster, is rich in nebulæ. But Ursa Major, a region of the heavens of exactly opposite character—a region in which many brilliant stars shine on a background which in places appears (to me at least) of unusual blackness—includes also a rich part of the nebular stream. Leo Minor exhibits several small stars, but in this constellation nebulæ are rich where small stars (fourth magnitude) are wanting. In this neighbourhood the background may almost be termed nebulous. The rich nebular cluster in Virgo lies on that part of the constellation which borders on Coma Berenices. The region is surrounded on every side by stars of the third and fourth magnitude, but over the region itself stars are rather markedly absent. Crossing the equator, we find that the stream shows a decided preference for regions in which visible stars are scarce. Thus over Corvus and Crater, where lucid stars are closely clustered, there is a gap in the nebular stream, and the rich cluster near the break in the Milky Way occupies the blank region mentioned in the preceding essay (see page 9)

—a region than which there is none more desolate (so far as lucid stars are concerned) in the whole heavens. The neighbouring region, crossed by the brightest part of the Milky Way, and by a brilliant array of lucid stars, exhibits but few nebulæ.

Turning to the less-marked nebular cluster, whose greatest condensation is in Cetus, we find in its northern part a rich display of nebulæ over parts of Perseus and Andromeda, in (or around) which lucid stars are thickly clustered. Near the equator (on the southern side) we find nebulæ distributed richly over a region in which are many stars of all magnitudes, including many double-stars, and one noted variable. Of the two streams into which the nebular system here divides, one follows the windings of Eridanus, with an apparent preference for the spaces clear of stars around which that star-stream travels; the other forms itself into a cluster which occupies a space almost as blank of lucid stars at that between Hydra and the Milky Way.

When we consider those regions of the heavens in which nebulæ are markedly deficient, we find an arrangement which cannot be wholly accidental. I refer to the zone, very marked in the northern hemisphere, and not indistinctly traceable in the southern, which has been already described. This zone is not quite coincident, in direction, with the Milky Way, but follows almost exactly a circular band, which includes more lucid stars than any corresponding band on the heavens. In fact, if we neglect nebulæ which may fairly be considered to belong to the Milky Way itself, we may fairly say that the zone includes Canis Major, Orion, Taurus, Auriga, Perseus, Cassiopeia, the richer parts of Cepheus and Draco, Lyra, Scorpio, and finally, the rich region in Argo already mentioned.

Now, it appears to me that those who have speculated on the subject of nebulæ have been too apt to content them-

THE NEBULÆ and CLUSTERS of Sir J Herschel CATALOGUE

[illegible] Projection

NEBULÆ

CLUSTERS

RES [illegible]

DRAWN BY [illegible] WATERS

The Northern Milky Way [illegible] the Southern from Sir J Herschel selves by looking for zones and streams of aggregation, not noticing apparently that zones along which nebulæ are sparingly distributed, may be as marked indications of systematic distribution as zones of aggregation. I consider that the zone mentioned in the preceding paragraph is a phenomenon scarcely less distinct in character than the zone of the Milky Way itself; and I look on the connection between the former zone and the zone of brilliant stars as a very noteworthy circumstance.

I may remark in this connection that much less stress than the nature of the phenomenon fairly warrants, has been laid on the peculiarity that scarcely a single lucid star occurs within the gaps and cavities in the Milky Way. While stars are clustered in the borders of these spaces, *within* there is such a vacancy that one cavity is called the Coalsack, another is called by Sir W. Herschel 'an opening into space,' and Sir John Herschel, speaking of the vacant region near *a* Centauri, tells us that in his powerful telescope he found several fields wholly blank. There is not a single star of greater magnitude than the fifth within a cavity in the Milky Way, or between the two streams where the galaxy is double; and there are not ten stars of the fifth magnitude so situated. This will appear the more remarkable when we remember that, according to the stratum theory of the Milky Way, these regions ought to be very rich in stars of all magnitudes.

It has been already noted by astronomers that all nebulæ of irregular form and great extent are found along a zone nearly coinciding in direction with the Milky Way. The great circle along which such nebulæ are actually found is, in fact, no other than that along which nebulæ in general are conspicuously wanting. It is also worth noticing that where this zone, and the zone of the Milky Way intersect, we find the singular nebula round η Argus in one hemisphere, and in the other the remarkable nebular region in Cygnus.

The discovery that the great irregular nebula in Orion is gaseous, renders it probable that the other irregular nebulæ are so likewise. Whether they are so or not, it is clear that they are totally different in character from regular nebulæ. Therefore, we may look on their aggregation on the great circle along which few nebulæ are found as a circumstance (1) not opposed to the evidence of systematic distribution founded on that peculiarity; (2) as itself indicative of a law associating nebulæ with the stellar system.

One peculiarity of the irregular nebulæ deserves to be especially dwelt upon. All of them exhibit a tendency to arrange themselves around fixed stars. Consider, first, the nebula in Argo. In the first place, there is the actual aggregation of stars near the very region of the nebula—a fact which is *per se* remarkable, though not alone sufficient to indicate the connection I am seeking to exhibit. 'It is not easy,' says Sir J. Herschel, 'for language to convey a full impression of the beauty and sublimity of the spectacle which this nebula offers, as it enters the field of view of a telescope fixed in Right Ascension, by the diurnal motion, ushered in as it is by so glorious and innumerable a procession of bright stars *to which it forms a sort of climax.*' '*One other bright and very remarkably formed nebula of considerable magnitude precedes it nearly on the same parallel, but without any traceable connection between them.*' I have italicized this last quotation, (1) because it points to a phenomenon observed in all the irregular nebulæ, of associated but not always distinctly connected nebulæ; (2) because the association of *two* irregular nebulæ with the same stream of bright stars is well worthy of attentive notice.

But it is when we examine the features of the nebula that its association with the starry background becomes most clearly apparent. In the small map of the nebula given in Herschel's Astronomy the relation is not satisfactorily presented, but in the large map of which the former is a reduction we find the following peculiarities :—

The central and most condensed part of the nebula exhibits a vacuity of singular, but regular form, described by Herschel as a lemniscate-oval, but in reality more complex than this description implies. The brilliancy of the nebula is much greater on one side of this vacuity than on the other. In the middle of the bright region is situate the remarkable variable η Argus—a star marked in Halley's Catalogue as of the fourth magnitude, but which has been known to surpass Canopus, and even to approach Sirius in magnitude. Around the vacuity there are marked in no less than twelve stars, either exactly on, or very close to the border. One of these is just within the vacuity; but besides this star, which may fairly be termed a border-star, there is not a single star within the opening. On the other hand, there may be counted, besides the twelve stars above named, no less than fifty-four stars on the nebulous region around the opening. From the central part of the nebula there extend a number of nebulous streams, 'of whose capricious forms and irregular gradations it would be impossible to give any just idea.' But one peculiarity is noticeable throughout the convolutions and contortions of these irregular streams —viz., that *on* the nebulous portions stars are common, on the parts *clear* of nebula stars are either wholly wanting or very sparingly distributed. And those stars which do occur in apparently blank regions, yet appear evidently associated with nebulous streams and projections pointing directly towards them. The connection is so marked that I cannot understand how it can have escaped the notice of so sagacious an observer as Herschel; or rather how, noticing the phenomenon, he should not have recognised the evidence it afforded of a connection between the nebula and the fixed stars seen in the same field. There is not a single remarkable condensation or projection in the nebula which is not marked by bright or clustering stars, by stars which appear clearly to be *leading stars*, and there are not ten stars out of some hundreds entered, whose influence on the nebula is not clearly discernible. If the fainter portions of the nebula were examined with more powerful telescopes, I believe that even these stars would be found to be associated with the nebula.

As respects the great nebula in Orion, I remark that, while the proportion of stars whose connection with the nebula has not been traced is somewhat greater, yet nearly every marked condensation in the nebula is associated with conspicuous stars (that is, with stars conspicuous among those visible in the same field of view with the nebula). Every telescopist also is familiar with the fact, that the central condensation of the nebula clusters round the trapezium of stars, within which there is either no nebulous light or very little. The association is not likely to be an accidental one. Herschel himself remarks that the star ε Orionis is involved in strong nebulosity, 'probably connected with the great nebula,' while he was able to trace a faint extension of the nebula as far as the double star ι Orionis, which it involves, so that the star is rendered nebulous.

Turn we now to the region of irregular nebulæ of Sagittarius. The first nebula mentioned by Sir J. Herschel is described by him as 'singularly trifid, consisting of three bright and irregularly formed nebulous masses, graduating away insensibly externally, but coming up to a great intensity of light at their interior edges, where they enclose and surround a sort of three-forked rift, or vacant area, abruptly and uncouthly crooked, and quite void of nebulous light.

Plate VIII

THE STARS to 6½ Magnitude in the B.A. CATALOGUE
(Equal Surface Projection)

DRAWN BY RICHARD A. PROCTOR.

The Northern Milky Way from Heis, the Southern from Sir J. Herschel

A beautiful triple star is situated *precisely* on the edge of one of these nebulous masses, just where the interior vacancy forks out into two channels. A fourth nebulous mass spreads like a fan or downy plume *from a star*, at a little distance from the triple nebula.' Another (δ Messier), he describes as 'a collection of nebulous folds and masses, surrounding and including a number of oval dark vacancies, and in one place coming up to so great a degree of brightness as to offer the appearance of an elongated nucleus. *Superposed upon this nebula, and extending in one direction beyond its area, is a fine and rich cluster of scattered stars, which seem to have no connection with it, as the nebula does not, as in the region of Orion, show any tendency to congregate about the stars.*' Herschel gives a view, however, of the nebula and star-cluster which, it is not too much to say, is wholly irreconcilable with the opinions here expressed. Not only are the two brightest stars of the cluster placed exactly upon the 'elongated nucleus,' but every 'fold and mass' of the nebula is associated with a region of greater richness in the cluster.

As respects the fourth and last nebula, that of Cygnus, I may simply quote Sir J. Herschel. He describes the region as 'consisting, first, of a long, narrow, curved, and forked streak, and, secondly, of a cellular effusion of great extent, *in which the nebula occurs intermixed with, and adhering to, stars around the borders of the cells, while their interior is free from nebula, and almost so from stars.*'

I have already drawn out this paper to a much greater length than I had proposed, and yet seem scarcely to have entered upon my subject. Let me, instead of proceeding to treat cursorily of the remaining branches of that subject, here pause and 'report progress.' We have found a law of aggregation of nebulæ in regions removed from the Milky Way, and thus a law of contrast, which amounts in reality to a law of connection between nebulæ and the starry system. We have found that, in the southern hemisphere, this law of contrast is further exhibited in an aggregation of nebulæ over regions in which stars are wanting, and *vice versâ*; lastly, we have seen that over a *zone* of the heavens in which nebulæ are all but absolutely wanting, there is a marked aggregation of lucid stars, that on the same zone all the irregular nebulæ are collected, and that these irregular nebulæ, all occurring in regions very richly bestrewn with fixed stars, exhibit in their configuration a correspondence with the configuration of the fixed stars in the same field, which cannot be wholly accidental.

From the *Student* for March 1868.

A NEW THEORY OF THE UNIVERSE.

THE present century has been remarkable for the progress which has been made in all departments of astronomy. Within the solar system, within the sidereal or galactic system, and within the yet wider range ascribed to the nebular system, discoveries of the most important character have been effected. There is a singular contrast, however, between the amount of positive knowledge which has been deduced from observational labours within the solar domain, and the somewhat vague ideas which astronomers are content to hold respecting sidereal space. I shall endeavour to exhibit the fulness of this contrast, and then to point out some of the more remarkable consequences which seem to flow from modern observations within the stellar and nebular domains.

At the end of the last century astronomers recognised in the solar system a mechanism of an uniform and symmetrical character. Around a central orb they saw revolving a family of dependent globes, vast in their absolute dimensions, but minute in comparison with the massive globe which sways their movements. Amongst these bodies they saw several attended upon by yet smaller globes, forming secondary systems, which resemble in many respects the great system of which the Sun is the controlling centre. The late discovery of Uranus had led them to recognise the possibility that beyond the known planets there may exist others, perhaps by no means the least important members of the solar system. Little was known, however, that differed in kind from what had been known to Aratus, Hipparchus, or Ptolemy. When we have named the ring of Saturn and a few periodic comets, which were looked on rather as accidental solar attendants than as forming a normal feature of the system, we have mentioned all that the three last centuries had revealed which differed in character from what had been recognised for two thousand years.

Very startling is the contrast when we turn to consider the views at present held respecting the solar domain. We no longer see a system which, however complex, might yet be very adequately represented by human mechanisms. We recognise, within a sphere exceeding manifold in diameter the orbit of distant Neptune, a variety and complexity of formation of which the human mind is unable to form adequate conceptions.

The increase in the number of primary attendants upon the Sun, though far from being the most remarkable discovery which has been made during the present century, is well worth dwelling upon for a moment. Lately the 98th asteroid was discovered (see date of this essay), and yet it was but on the opening day of the century that the first of these bodies was discovered. In these new members of the solar system we recognise characteristics which had not hitherto been presented to the notice of astronomers. We see a series of bodies, primaries of the planetary system, which yet, instead of travelling in distinct and widely-separated orbits, revolve in paths closely interwoven. Even when but forty had been discovered it was truly said that if each orbit were represented by a hoop, it would be impossible to lift any one of these hoops without lifting the whole set. We may fairly assume that for each discovered asteroid there are to be reckoned tens, perhaps hundreds, which will remain for ever undiscovered.

It has been found, also, that there exist within the solar system myriads of dependent comets. Revolving around the Sun in orbits of the most varied figure, differing among themselves in size and character, and presenting—some of them—the most singular phenomena that have ever rewarded astronomical observation, these objects remain among the mysteries of science. The only two which have as yet been submitted to the searching analysis of the spectroscope are found to consist of a gaseous nucleus attended by a coma which probably shines by reflected light; but whether this is the case with all or even the generality of comets it would be assuming too much to assert.

The most remarkable feature of modern astronomical discovery remains yet to be mentioned. A phenomenon which men had long been in the habit of looking upon as a meteorological one has been at length recognised in its true light, and has been found wonderfully to enhance our appreciation of the complexity of the systems which exist within the solar domain. Meteors, shooting-stars, and aerolites have taken their place among the attendants of the Sun; and in several instances the orbits they have followed before they reached the Earth have been approximately determined.

But it is rather as members of systems than as individual bodies, that these objects acquire their chief interest and meaning. There was not much, perhaps, to attract attention to them when they were supposed to form one or two rings occupying a position in space very nearly coincident with that of the Earth's orbit. But it has now been placed beyond a doubt that the Earth encounters fifty-six systems, at least, of these small bodies. And these systems are found (in the only instances yet examined) to be—not circular rings—but ovals of great eccentricity extending far into space—even in some cases beyond the orbits of Uranus and Neptune. It is clear, then, that we can no longer look on these systems as resembling, in the remotest degree, the asteroidal zone. We are forced, too, to take into consideration an important question of probability. What is the likelihood that if there were but a few hundreds of such systems, the Earth would encounter so many as fifty-six? (See date of article). The probability may be reckoned 'almost at naked nothing.' And therefore we are compelled to admit as a legitimate *à posteriori* deduction, the extreme probability, we may almost say the certainty, that such systems are to be reckoned—not by hundreds and thousands—but by millions on millions.

Nor is this all. Within the last few months the startling discovery has been made that two of the meteoric systems at least, and probably many others, coincide throughout their calculated extent with the orbits of known comets. Accordingly we are led to trace an intimate connection, if not an absolute identity, between comets and shooting-star systems. And when we find that a system, which has afforded such grand displays of star-falls as the well-known November shooting-star system, is identified—not with a large and conspicuous comet—but with one which has only lately been detected, though it must have been in close proximity to the earth some thirty times during the last thousand years, with a comet, in fact, which is absolutely invisible to the naked eye, and far from being a conspicuous object in powerful telescopes, we are led to recognise the importance of such comets as Newton's, Halley's, and Donati's.

The result to which these considerations lead is clearly this:—

The interplanetary spaces, so far from being looked upon as untenanted, save by an occasional wandering comet, must be considered as crowded with various forms of cosmical matter. I would not be understood as using the term 'crowded' in a sense implying absolute proximity between the various members of the cometic or meteoric systems. On the contrary, the evidence that we have assures us that the sum of the volumes of all the members of a system must bear an indefinitely small proportion to the total space occupied by the system. But if an eye, armed with new powers of vision, and placed at some far distant point, could see at one glance all the systems which occupy the solar domain, they would appear as a complicated network formed by interlacing streams of cosmical dust. And amidst the streams of misty light representing cometic or meteoric systems, the planets would shine forth as distinctly and as brilliantly as the brighter stars upon the background of the Milky Way.

Nor does it seem difficult to determine the general laws according to which the density or compactness of the interwoven streams would seem—to an eye placed as we have supposed—to vary with distance from the central orb.

Proceeding from the outermost parts of the system towards the Sun, we may conceive that there is in the more remote regions a gradual condensation, but that this increase of density becomes much more rapid in the immediate neighbourhood of the Sun. This is not the only law, however, according to which the density of matter distributed throughout the solar domain must be supposed to increase. There is a certain plane near which all the primary members of the solar system are observed to move. In the asteroidal family, whose members depart more freely than the larger planets from this great central plane, there is, nevertheless, a distinctly marked obedience to the general law of aggregation in its neighbourhood. Three-fourths of the asteroids revolve in orbits less than ten degrees inclined to the medial plane, and there are not ten of them whose orbits are inclined so much as twenty degrees. Now, on a first view of cometic relations, we notice scarcely any trace of a tendency to aggregation near the medial plane of the solar system. Nay, so far is this from being the case, that among several hundreds of comets whose orbits have been determined, a decided tendency of a very different character has been noticed. If we suppose the Sun to lie at the common vertex of a double cone having a semi-vertical angle of forty-five degrees, and its axis perpendicular to the medial plane, then the planes of cometic orbits exhibit a tendency to present themselves as tangent-planes to this imaginary cone.[1] But when we consider cometic orbits more closely, we find abundant evidence of a tendency amongst those comets which are nearest to the Sun to aggregate around the medial plane of the solar system. There are some twenty comets which have been recognised as travelling within the orbit of Saturn. Among these there are only two whose orbits are inclined more than fifteen degrees to the medial plane of the solar system. Now there is no reason whatever for supposing that there are not multitudes of undetected comets whose perihelia lie far nearer to the Sun than any yet discovered. On the contrary, we have distinct evidence of a rapid increase in the number of perihelia, with decrease of distance down to and within the neighbourhood of the Earth's orbit;[1] and, remembering the probability that comets whose perihelia lie nearer to the Sun would escape observation altogether, we have every reason for supposing that this law of increase is continued—as why should it change?—right up to the immediate neighbourhood of the Sun. And further, we may confidently assume that that obedience to planetary laws which, as we have seen, begins to be exhibited by comets within the orbit of Saturn, becomes yet more marked among comets nearer to the Sun. Therefore, it seems highly probable that cometic orbits, and especially those which are nearest to the Sun, show a marked tendency towards aggregation near the medial plane of the solar system.

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[1] This tendency has been pointed out by one of our most distinguished modern astronomers. As it seems impossible to suggest any rational explanation of so remarkable a peculiarity—for we have to explain, not merely the fact that the orbit-planes show no tendency to coincidence with the medial plane, but also the fact that the medial plane should be connected *in any way* with cometic orbits—one seems permitted to question whether the peculiarity is real, or only apparent. Now, if we remember that, *cæteris paribus*, the greater the inclination of a comet to the plane of the ecliptic (virtually coincident with the medial plane of the solar system) the greater the antecedent probability that the comet will be detected, we may recognise a cause for the observed peculiarity, independently of any real peculiarity in the arrangement of cometic orbits. A gradual diminution in the number of orbits as we leave the plane of the ecliptic, in combination with this gradual increase in the probability of detection, might very well lead to such a result as we have specified. According to the law of decrease or increase respectively, the variation in the numerical distribution of observed cometic orbits might point to the existence of a maximum at *any* assigned inclination to the ecliptic.

The tendency in question is so far from being strongly marked that this consideration may, for the present, be held to be a sufficient explanation.

A celestial phenomenon, of which we have not hitherto spoken, appears to gain a far easier explanation from the considerations above adduced, than from the theories ordinarily adopted respecting it. The zodiacal light has been accounted for in three ways. There are some who hold that it is an atmosphere of the sun; others that it consists of a ring of cosmical particles, travelling around him in a nearly circular orbit; and others that it consists of a lenticular disc of cosmical dust, each portion of which travels in a nearly circular orbit. The remarkable phenomena presented by the zodiacal light, its strangely fluctuating figure, its varying position, and the singular increase and diminution noticed in its distinctness, are not accounted for by any of these theories. But if we recognise in the zodiacal light merely the effect of the above-considered aggregation among the cometic or meteoric systems which exist within the solar domain, the variations I have mentioned become readily explicable. A multitude of bodies travelling in orbits of every degree of ellipticity and magnitude, but with a marked aggregation in the neighbourhood of the Sun, and with a yet more marked aggregation in the neighbourhood of the medial plane of the solar system, would, in the first place, exhibit precisely such an appearance as the zodiacal light; and, in the second place, the general illumination resulting from the congregated comets would be liable to continual variation. Comets would be continually arriving within and passing away from the region within which their light would assist in forming the appearance we are considering. At one time the press of arrivals would temporarily increase the density of cometic aggregation; at another, the reverse would hold for a while, and the zodiacal light would wax and wane accordingly, precisely as it is observed to do. So also its figure and apparent position would be liable to changes corresponding to those which are actually presented. Therefore, without denying positively that the zodiacal light is caused by the existence of a multitude of minute bodies travelling in orbits of small eccentricity around the Sun, I hold that the phenomena correspond far more closely with those which would be presented if there is in the neighbourhood of the Sun a great increase in the density with which cometic and meteoric systems are congregated together in the neighbourhood of the medial plane of the solar system. And this correspondence becomes a strong argument in favour of such an increase of density when it is remembered that, as we have seen, there exist independent reasons for believing an aggregation of this sort to be not only possible, but highly probable.

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[1] Thus, out of one hundred observed comets, fifty-eight have perihelia between forty millions and a hundred millions of miles from the Sun, twenty have a less perihelion distance, and four only have a perihelion distance exceeding one hundred and sixty millions of miles.

But whatever opinion we may form on this and kindred questions, there is no dubiety whatever about the general results which have been presented above. Our conceptions of the solar domain are different, indeed, from those formed of old. 'There was true prophecy,' as has been well remarked by the late Professor Nichol, 'in the exclamation of Laplace, who, although knowing more of the celestial mechanism than any man then living, said earnestly, on his death bed, "That which we know is little; that which we know not is immense."'

When we turn to examine the views which were held respecting the sidereal system at the commencement of the present century we find that they are distinguished by no very marked points of difference from those at present entertained. Yet have many important discoveries been made in the interval, which seem to suggest a modification, in many respects, of the views which have so long held their ground. What these are I proceed to point out.

So soon as the Copernican theory had become thoroughly established, and had been supplemented by adequate conceptions of the dimensions of the Earth's orbit round the Sun, it became manifest that the stars must be placed at an enormous distance from the solar system. That the motion of the Earth in an orbit one hundred and eighty millions of miles in diameter, should produce no appreciable effect on the configuration of the constellations, could be explained in no other way than by supposing that an orbit of these dimensions, viewed from the nearest fixed star, would scarcely present appreciable proportions. And when the nicest observation with the most accurate instruments which were then procurable, showed that any parallactic displacements which may exist among the stars, are insensible, or, at any rate, fall short of recognised instrumental errors, astronomers were compelled yet further to extend their conceptions of the immensity of the interval which separates the Sun from the nearest fixed star.

Accordingly, the notion that each visible star may be a Sun, in magnitude and splendour equal to, or perhaps excelling our own, was early recognised by astronomers as not merely reconcilable with the apparent minuteness of the stars, but as suggested by a comparison of the brilliancy of their light at the enormous distance we have referred to, with the splendour of the great luminary of our system. And it was seen that the great variety of lustre which is observed among the stars is no evidence of any corresponding variety in their real magnitudes, but may be readily explained by the supposition that the stars are placed at different distances from the Sun. Perhaps astronomers in this respect fell into the opposite error, and were too ready to assume diversity of distance as the sole, or almost the sole, explanation of difference of lustre. 'The supposition,' says a modern writer, 'has been usually adopted; and we accordingly consider the stars to derive their variety of lustre *almost entirely* from their places in the universe being at various distances from us.' Here we may apply the first lesson which is taught us by a consideration of the solar system; and from the analogy of that system, with the infinite diversity of magnitude presented among its various members, we may learn to expect a corresponding diversity among the components of the great sidereal system.

When the considerations which had been applied to the scattered stars visible either to the naked eye or with the telescope, came to be extended to that vast irregular annulus of nebular light called the galaxy, or Milky Way, the most startling conceptions were suggested of the enormous extent of the sidereal system. This remarkable zone had from the earliest ages engaged the attention of astronomers. Long before Galileo had resolved portions of it into stars, Democritus had maintained just views respecting its structure. Manilius also suggested—

> An major denså stellarum turba coronå
> Contexit flammas, et crasso lumine candet,
> Et fulgore nitet collato clarior orbis.

Regarded, however, as a *zone of suns*, this phenomenon acquired a new and astounding significance. If we could suppose a multitude of suns resembling our own to be so closely compacted together as the component stars of the Milky Way appear to be, there must result an inconceivable splendour in those far distant regions; if, on the other hand, the orbs which seem to lie in such close order, are in reality separated by distances comparable with those which separate the Sun from the nearest fixed star, how inconceivably distant must they lie from us, that such intervals as these should be diminished to evanescence! The last supposition has been the one universally accepted by astronomers. Of the two it clearly accounts best for the observed appearance of the galaxy. I shall presently have occasion to show the probability, if not the certainty, that neither view represents the relations which actually subsist among the stellar components of the Milky Way.

The phenomena presented by this zone of nebulous light are intimately associated with the remarkable researches of Sir William Herschel among the fixed stars. This eminent astronomer, in whom were presented all the qualifications required to constitute a first-class observer, side by side with that power of systematic reasoning on observed facts which so seldom accompanies the highest observing powers, was early fired by the bold ambition to gauge the depths of our sidereal system. He cast aside the notion, which had been held, almost unquestioned, till his day, that the stars of that system extend on every side to an infinite distance. He saw that the existence of a Milky Way affords evidence that the sidereal system has definite bounds; and he quickly grasped at the only method which is available for the determination of its figure. 'This great, inspired, and cautious observer,' says Humboldt, 'first cast the plumb-line into the depths of heaven, to determine the boundaries and the form of the separate cluster of stars which we inhabit.' It has been said of him that 'he broke through the barriers of the heavens (*cœlorum perrupit claustra*),' and we shall presently see in what sense these words have been used. But it must not be forgotten that to his labours and to those of his son are due the ideas we at present hold of what those barriers are. He was at once the Romulus and the Remus of astronomy; he marked out the limits of our system, and he showed how man might boldly venture beyond those limits into the domain of the illimitable.

Herschel's method of gauging the sidereal heavens has been long and deservedly the theme of admiration. The boldness and originality of the conception, and the unwearying perseverance with which the laborious processes involved were carried out—by the elder Herschel over the northern hemisphere and by the younger over the southern—are unexampled in the history of observational astronomy. Assuming a certain approach to uniformity in the distribution of the stars, and also (for this is very important) that there is no such law of extinction of light in traversing great distances as would prevent a telescope of great power from penetrating the full depths of the system in every direction, it is clear that a very simple process will serve to indicate the relative distance of the observer from different parts of the system's exterior surface. This method and its results have already been discussed in the pages of a preceding essay. We have seen that the labours of the two Herschels seem to show that the stellar system forms a flattish disc of stars, whose central plane corresponds with that of the Milky Way. In one direction this disc is cloven, and those parts of the heavens which lie opposite the two divisions of the stellar disc are occupied by a double stream of milky light.

We have seen also that Herschel soon recognised a complexity in the structure of the Sidereal System, which prevented him from regarding the figure of a cloven disc as any save the roughest representation of the galactic system. He saw that in portions of the Milky Way the stars exhibit a tendency to form themselves into clustering groups, and he saw that a tendency of this sort would be quite sufficient to vitiate, not merely those gauges which were made in the direction of the clustering stars themselves, but also those made in neighbouring regions; for where the stars were clustering together, the star-gaugings would indicate a depth not really presented by the sidereal system in that direction, and the regions in which stars were more sparsely strewn (owing to the influence of neighbouring aggregations) would also give false evidence, but of an opposite kind, respecting the depth of the system.

Some of the peculiarities presented by the Sidereal System must now engage our attention.

In some regions of the Sidereal System extremely minute stars are seen, so sparsely distributed as to fall very far short of the number which ought to be seen if the system extends to the distance indicated (on the usual theories) by the minuteness of these objects, very far short, in fact, of the number actually seen in other fields not more richly strewn with stars of less minuteness. We seem compelled therefore to assume, either that these very minute stars are actually intermingled with the larger stars which appear in the same field, or that the galactic system thins off rapidly near its edges. In other words, we must either abandon the notion of any approach to uniformity in star-magnitudes, or we must concede a want of uniformity in stellar distribution, near the boundaries, at any rate, of the galactic system.[1] We are, of course, also free to assume a want of uniformity in *both* respects.

In other regions a yet more noteworthy phenomenon presents itself. The stars seen over the ground of the heavens exhibit an almost uniform lustre, and at the same time a very even distribution. In such a case we are bound to assume a *real* uniformity, both in the magnitude of the individual stars and in the distances which separate them. For the actual uniformity of apparent magnitude can only result from a real uniformity, or from such a distribution of unequal stars at distances proportioned to their magnitude as is wholly improbable. And it is in an equal degree improbable that, if the distances between the component stars were unequal, a peculiarity of arrangement should result in presenting to us the appearance of uniform distribution. It appears to me that there is no way of escaping from the conclusion accepted by Sir John Herschel, that we are here 'looking *through* a sheet of stars nearly of a size, and of no great thickness compared with the distance which separates them from us.'

In other cases a double phenomenon of the kind just described is presented, leading to the conclusion 'that in such cases we look through two sidereal sheets separated by a starless interval.'

It is hardly necessary to point out that the uniformity evidenced by these peculiarities is of a very different kind from that which the elder Herschel took as the basis of his star-gaugings. His results were obtained on the supposition that there are no such starless intervals as must necessarily be assumed to lie on either side of the 'star-sheets' of Sir John Herschel.

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[1] It is singular that Sir John Herschel omits all notice of the second, and perhaps the more probable alternative. 'In those regions,' he writes, 'where the Milky Way is clearly resolved into stars well separated, and *seen projected on a black ground*' (the italics are his), 'and where by consequence it is *certain* that we look out beyond them into space, the smallest visible stars appear as such, not by reason of excessive distance, but of a real inferiority of size or brightness.

Another law has been detected in the aggregation of stars, a law which tends rather to modify than to negative the results of Herschel's star-gauging. Among the *possible* explanations of the existence of a galactic zone, there is clearly this one,—that, in place of a great extension of our system in the direction indicated by that zone, there exists a real condensation of stars in that direction. To determine whether such a condensation, or a condensation of any sort, exists in the neighbourhood of the Milky Way, it is necessary to compare not merely the number of stars visible in different fields of view, but the number of stars of the same apparent magnitude. That is, it is necessary to take into account the relative distance of the stars as supposed to be indicated by their relative lustre. This has been done for all orders of stars down to the ninth, inclusive, by the elder Struve. He has shown that there is a marked condensation of stars towards the plane of the galactic circle; a condensation, however, which, as might be anticipated, is far from being sufficient, *per se*, to account for the observed increase of light in that direction. In other words, we are still bound to accept a considerable extension of our system in the galactic plane as fairly deducible from the gauges of Sir W. Herschel, but we have a secondary cause for the law observed in those gauges which is not to be neglected when we are attempting to realize the actual constitution of the sidereal system. It may be noticed in passing that the condensation detected by Struve is more marked among the smaller stars; but is not wholly wanting among the stars of the first four or five magnitudes. Sir John Herschel's gauges in the southern hemisphere gave a different result, and he holds that it is only as we approach the sphere of the telescopic stars that we obtain any appreciable indications of condensation. The difference between the two hemispheres in this respect is worth noticing, and may be looked upon as connected with another peculiarity mentioned by Sir John Herschel, viz., the greater richness of the southern hemisphere in stars of all orders.

We see, then, that in many important respects Herschel's first supposition, that there exists an approach to uniformity in the magnitudes of stars, and in their distribution throughout our system, is found not to be consistent with observed facts. Let us next consider his second supposition, or I believe we should be more correct in saying his tacit assumption, that there is no appreciable extinction of light in traversing interstellar space. The considerations connected with this question are of great importance and interest.

It may seem at first sight, and has been maintained by many eminent scientific men, that if infinite space is occupied by an infinite number of stars, whose average intrinsic lustre is equal to that of our own Sun, then, unless light suffer extinction in its progress through celestial regions, the whole sky would everywhere appear as brilliant as the solar disc. Indeed it may be *proved* that an uniform distribution of such suns, at whatever common distance, and whatever their mean magnitude, must inevitably lead to an illumination of this sort unless light be gradually extinguished as supposed. Or again, if we suppose that the system of stars whereof our Sun is a member is limited on every side, but that an infinite number of such systems exist uniformly distributed, at whatever common interval, throughout space, the same result would inevitably follow. Now, wholly incomprehensible as the idea of infinite space is to our finite minds, we cannot escape the admission of its existence.

The only question for us is between an infinity of occupied space, and an infinity of vacant space, surrounding a finite universe. Either idea is equally incomprehensible; but the former is merely *beyond*, the latter seems *contrary* to reason. It would seem, therefore, as if we were *compelled* to accept the gradual extinction of light as the only possible explanation of the darkness of the background on which the stars appear to be projected.

But there is another way in which we may explain the darkness of the sky at night, without assuming either the extinction of light, or that occupied space is an infinitely minute speck amidst an infinity of vacant space. So far as I am aware, it is new, at least as regards its application to the difficulty we are now considering: In the solar system we are presented with a finite number of bodies placed within a region of unoccupied space, bearing a very large ratio to the dimensions of the solar domain. In the sidereal system we seem to be presented with a finite number of such suns, forming a cluster, which is surrounded on all sides by unmeasured depths of space. Assuming our system to form one of a *finite* number of similar systems, separated from each other by distances bearing a very large ratio to the dimensions of each, and that thus a system of a higher order is formed, which again forms one of a *finite* number of similar systems, and so on continually,—*the dimensions of each system of whatever order being always very small in comparison with the distance separating it from its neighbours*,—then there would no longer result as a necessary consequence even an appreciable illumination of the whole heavens. I do not present these views as hypotheses which I wish to support; all I seek to show is that, by assuming the existence of such systems in successive orders, the subject of the extinction of light is left an open one. Of course the question is also left open if we assume that occupied space is finite.

Such an assumption appears to me, however, to be inconsistent with the conception of an omnipotent Creator.

The arguments for and against the extinction of light have an important bearing on the theory of our sidereal system. Few subjects have been more acutely and closely reasoned.

Olbers was the first, I believe, to put forward a definite theory asserting the extinction of light, though Halley, and many other philosophers, had held somewhat similar views. In support of Olbers' theory the elder Struve put forward the following considerations:—

Sir W. Herschel's twenty-feet reflector should have a space-penetrating power of 74·83, the range of the naked eye being taken as unity. Now Struve found by a computation based on the law of condensation of the stars in the plane of the Milky Way, that such a power as this would result in giving an average of 3,021 stars for the telescope's field of view, 15′ in diameter. But, instead of this number, Herschel's observations give only 122 stars per field. And Struve calculated that Herschel should have seen as many stars as this with a space-penetrating power of only 25·67, if there is no extinction of light. It seemed to follow, therefore, that the minutest stars visible in Herschel's telescope send no more light to us than stars nearly three times as far off would send if there were no extinction. Therefore the smallest stars visible to Herschel shine—according to Struve—with little more than one-ninth part of their proper lustre; or, more exactly, the diminution of brightness is no less than $\frac{88}{100}$ths of the whole quantity.

But it is answered by Sir John Herschel that the results on which Struve founds his calculations may be otherwise explained. If there is a diminution in the density of stellar aggregation towards the limits of the Milky Way, corresponding to the observed diminution at right angles to the plane of that zone, the same effects would be perceived as would result on Struve's hypothesis of an extinction of light.

To this Struve answered that the diminution of density must—in order to account for the results of observation—be assumed to take place on every side of the Sun, which there[illegible] must be supposed to occupy a central position in the sidereal system. But we have no evidence whatever—said Struve—in support of this supposition. He considered that the galactic system is unfathomable—so far as we are concerned. He therefore dismisses the supposition of a diminution in the density of star-aggregation, as altogether inadmissible.

He pointed out another objection to the hypothesis of a diminution of density in stellar aggregation. He found that, on the supposition of a stratum of stars of indefinite extent—that is, unfathomable to us—and uniformly diminishing in density on either side of its central plane, there results from Herschel's gauges a *law* of diminution according almost exactly with the law which results from an examination of the numerical relations among stars down to the ninth magnitude. He considers that this agreement cannot be accidental. It must be remarked, however,—before proceeding to positive objections to Struve's view—that a single agreement of this sort, though antecedently improbable, is not sufficient to establish such a theory as that for which Struve was contending.

But now an argument was brought forward which appeared almost conclusive against the views of Struve and Olbers. It was pointed out by Sir John Herschel that the Milky Way *is* fathomable in almost every part by the twenty-feet reflector. Therefore, on the one hand, the fact that in nearly every direction the richest portions of the Milky Way are resolved into discrete stars perfectly free from nebulosity, affords decisive evidence that the sidereal stratum is not infinite in extent. On the other hand, the fact that in certain directions the stratum has an increased range which prevents the twenty-feet telescope from completely resolving it, affords equally decisive evidence that light coming from a distance far beyond that which Struve regarded as the limiting range of such a telescope is rendered perceptible to us by its means. 'We are not at liberty,' Herschel justly points out, 'to argue that at one part of its circumference our view is limited by this sort of cosmical veil which extinguishes the smaller magnitudes, cuts off the nebulous light of distant masses, and closes our view in impenetrable darkness; while at another, we are compelled by the clearest evidence the telescope can afford to believe that star-strewn vistas *lie open*, exhausting their powers and stretching out beyond their utmost reach, as is proved by that very phenomenon which the existence of such a veil would render impossible,—namely, infinite increase of number and diminution of magnitude terminating in complete irresolvable nebulosity.'

It seemed, therefore, that as yet no evidence proved that light suffers extinction at all; and certainly light had not been shown to suffer appreciable extinction within the bounds of our sidereal system.

Hence it would follow that we must either accept Herschel's view that there is a rapid diminution in the density with which stars are distributed, as we leave the central regions of our system; or else we must suppose that there is a sensible diminution in the magnitude of stars around the borders of the sidereal disc. The latter view, which I believe to be new, seems at least as probable as the other. In either case we must assume that our Sun is not very far from the centre of the system.

There are other features of the sidereal system which are well deserving of a careful scrutiny. The existence of variable stars, and of binary and multiple systems, the sudden blazing forth of temporary stars, some of which have excelled even Sirius in splendour, the reputed existence of dark orbs, —all these and many other evidences suffice to show how much variety there is amongst the denizens of stellar regions. That there should be such variety is what our contemplation of the solar system leads us to expect. And indeed, when we remember how largely the resources of astronomy have been taxed for the detection of the known peculiarities of the solar system, and that, for aught we know, not a tithe of the various orders of bodies which form that system have yet been revealed to us, surely it seems little likely that what we know of the sidereal system affords the faintest conception of the wondrously varied forms of creation which doubtless exist within the stellar spaces.

But we have yet to consider those mysteries of mysteries —the nebulæ.

The discovery that the vault of heaven is bestrewn with multitudes of objects, which differ wholly in character from the fixed stars, may be said to belong to the last hundred years. In 1768, only 68 nebulæ had been detected. Singularly enough, 42 of these belonged to the southern hemisphere. In 1771, Messier published a list of 103 nebulæ. But all prior investigations sink into insignificance in the presence of the discoveries made by the two Herschels. In 1786, Sir W. Herschel published a list of 1,000 nebulæ; three years later he added another 1,000; and finally, in 1802, he formed a supplementary list of 500 new nebulæ. Sir J. Herschel examined no less than 2,306 northern nebulæ, of which 500 were discovered by himself; then journeying to the Cape of Good Hope, he formed a catalogue of 1,708 southern nebulæ. Of the 6,000 known nebulæ, the Herschels have discovered more than four-fifths.

The views entertained respecting nebulæ are so vague that it is hardly possible to present them in any systematic form. But perhaps we shall be able to form a tolerably fair estimate of modern notions by considering the two chief hypotheses which have been held by astronomers.

First we must draw a line of demarcation between nebulous objects which are assumed to belong to the galaxy, and true nebulæ. This is not so easy a matter as it might seem at first sight. In the sidereal universe we see binary, triple, and multiple systems; we see also star-clusters, such as the Pleiades, and Præsepe, and that wonderful cluster which adorns the sword-handle of Perseus; we see also clustering collections of stars, extending far more widely than any of these. But already we have touched upon a difficulty. Such spots of light as the cluster in Perseus—are these to be looked on as *certainly* belonging to the galactic system? I have no doubt myself upon the point; nor, so far as I am aware, has any astronomer ever expressed any. Yet it is not easy to see on what grounds we can assume that the spot in Perseus belongs to the Milky Way, while we look upon other clusters—the great cluster in Hercules for example—as lying far out in space beyond the confines of our galaxy. For the present, however, we may content ourselves with the distinction usually drawn by astronomers, and admit irregular and widely-distributed clusters as belonging to the sidereal system, while we place the globular and closely-compacted star-clusters in the list of true nebulæ.

Thus there remain five classes of nebulæ:—

Resolvable nebulæ, or nebulæ which exhibit such an appearance as leads the experienced astronomer to suspect that with an increase of telescopic power they can be resolved into discrete stars.

Nebulæ which exhibit no trace of resolvability.

Planetary nebulæ.

Stellar nebulæ.

Nebulous stars.

Now, according to one view, which was maintained until quite recently as by far the most probable theory, objects belonging to all these classes (including globular star-clusters), were looked upon as in reality composed of suns, resembling our own in magnitude and splendour, and separated from each other by distances comparable to, perhaps surpassing, the distances which separate our Sun from neighbouring fixed stars. Nebulæ, in fact, were looked upon as galaxies resembling our own, some exceeding it, others falling short of it, in richness and splendour: but all of them 'island universes,' to use Humboldt's expressive verbiage, and all of them—even the nearest—removed from us by distances which exceed, in an enormous proportion, the dimensions of our galaxy. The resolvable clusters were of course considered to be the nearest of the outlying universes. Accordingly, when it is remembered that some of these are clearly resolved by pigmy tubes, while there exist irresolvable nebulæ of great apparent extent, which have defied the power of the great Parsonstown reflector, it will be seen how largely—on the hypothesis we are considering—the 'island universes' vary in their distances from us, and in their own dimensions.

According to the other hypothesis, multitudes of the nebulæ are outlying universes, but not all of them. Sir W. Herschel, in the beginning of his career, had held the former opinion, following in this respect Cassini and Mitchell. But in the first year of the present century he began to express different views. Admitting that the majority of the nebulæ are aggregations of suns, rendered nebulous only through excessive distance, Herschel was led to the belief that many nebulæ are formed in reality—as in appearance—from cosmical vapour. Half a century before, Kant and Lambert had expressed similar views; but their speculations had not been founded, as Herschel's theories were, upon a long process of research among nebulæ. Herschel's treatment of his observations was characterised by his usual clear-sightedness. He pointed out the remarkable contrast that exists between the small yet easily resolved clusters, and such objects as the great Orion nebula, and the 'queen of the nebulæ' in Andromeda. Both of these are distinctly visible to the naked eye, and are yet absolutely irresolvable even in those monster tubes which the Herschels directed towards the heavens. He dealt also with the peculiarities of the planetary nebulæ and of nebulous stars. The former, shining as discs of bluish or greenish light, are very unlike star-clusters of equal apparent dimensions. In many respects also they present a very different aspect from that which we should be disposed to assign to star-universes of abnormally large absolute dimensions, removed to a proportionately enormous distance. As regards the 'nebulous stars,' there are equal difficulties to encounter. If the central brightness is really, as it appears to be, due to the presence of a single star, how largely must the dimensions of this sun exceed those of the other members of the system it belongs to; and how different, therefore, must that system be from our own galaxy. On the other hand, if the central brightness affords evidence of a close aggregation of stars, how different is the variety of distribution indicated by this arrangement from the uniformity attributed to our own sidereal system.

Influenced by considerations such as these, and by others on which I have not space now to dwell, Herschel propounded the hypothesis that many of the unresolved nebulæ are not sidereal systems, but are formed of a nebulous fluid resembling in some respects that which is assumed to form the substance of comets. He held, as a natural corollary to this view, the opinion that nebulæ thus composed are not necessarily far removed beyond the limits of our own galaxy, but may be situated amidst the interstellar spaces. He showed further how there may be traced among the different orders of vaporous or fluid nebulæ the stages of a process of development leading upwards, he held, to the formation of suns resembling our own.

I have said that the former theory has been held to be the most probable one. This has arisen from the fact that under the amazing light-gathering power of Lord Rosse's great reflector, and under the exquisite defining power of the Harvard College refractor, the Orion and Andromeda nebulæ have been resolved, in part at any rate, into discrete stars.

But the marvellous revelations afforded by the spectroscope, in the able hands of Dr. Huggins, have shown that the one great mistake into which it had been assumed that Herschel had fallen, was in reality one of the most remarkable of his many anticipations of modern discoveries. We cannot, indeed, assert that Herschel's speculations respecting the genesis of stars have been confirmed. They have not, however, been disproved. And the great fact which he considered as the legitimate deduction from his observations has been placed beyond a doubt. The spectroscope tells us, in a manner which admits neither of doubt nor cavil, that many of the nebulæ *are* composed of luminous gas, and amongst these are to be included the Orion nebula and all the planetary nebulæ which have as yet been observed. The Andromeda nebula, about which Herschel expressed no decided opinion, is found to shine with stellar light. The same is the case with all the cluster-nebulæ which have yet been examined with the spectroscope.

In the preceding paper on the distribution of the nebulæ I have exhibited the very remarkable and significant relations which they present when viewed as a system. The supposed nebular zone of Sir W. Herschel has been shown not to be a reality. On the contrary, the northern nebulæ are found to be gathered into a vast cluster covering about one-fourth of the surface of the northern celestial hemisphere, and situated nearly at the pole of the galactic circle. In the southern hemisphere there is a greater uniformity of distribution, save where, within the Magellanic Clouds, nebulæ are found clustered even more closely than in the richest parts of the northern nebular cluster. These mysterious clouds, however, are considered by many astronomers to possess a character which separates them as well from the true nebular system as from the sidereal system. The reasons on which this opinion is founded are sufficiently remarkable *per se*, though they are justified, I imagine, by the theories astronomers have been so long content to hold respecting the universe: *The Magellanic Clouds*, it would seem, *are to be looked on as belonging neither to the nebular nor to the sidereal system, because they exhibit the characteristics of both systems.*

In dealing with the accepted views respecting the sidereal and nebular systems, I have treated the two systems separately. In fact, according to the received opinions—whether we take the theory of those who look upon all nebulæ as 'island universes,' or that of those who consider that some few are to be excepted—the sidereal system is but a member of the nebular system. Just as the Sun is one among the stars, so the Milky Way is held to be one among the nebulæ.

In presenting the views I have been led to entertain respecting the constitution of the universe, I shall consider the two systems together—for this reason, simply, that I believe them to form but one system.

I would not be understood to assert that all the nebulæ lie within the confines of the Milky Way. There may be some few which really are external systems. For instance, I think it not improbable that the spiral nebulæ are galaxies resembling our own. But that the majority of the nebulæ, and especially such objects as the great star-cluster in Hercules, are to be looked upon as external universes, I am disposed wholly to deny.

I think, indeed, that I shall be able at once to show the extreme improbability that even such an object as the Andromeda nebula is an external universe on the assumption that the accepted view of the sidereal system is the true one. Let us consider. In the Milky Way we are supposed to have an aggregation of suns separated from each other—throughout the whole extent of the galaxy—by distances comparable with the distance which separates our Sun from the nearest fixed stars. But so widely are the outer parts of the Milky Way separated from us, that—though composed in this manner—they appear even in our most powerful telescopes as mere patches of filmy light. Nay, there are streams of light so faint that, in the clear skies of the southern hemisphere, and examined by such an observer as Sir John Herschel, with a reflector eighteen inches in aperture, they appear but as a scarcely perceptible stippling or mottling of the telescopic field of view, so that, to quote Sir John's own words, 'the idea of illusion has continually arisen subsequently.' Now what the distance of these streams from us may be, we cannot say; nor is it important for my present purpose that we should be able to form any estimate on this point. It is sufficient to notice that this distance—whatever it may be—must be looked upon as certainly not greater than the radius of the great disc formed by the sidereal system. Such a disc would have to be removed to a distance many times exceeding its own diameter before it would present the dimensions of the Andromeda nebula or of the largest globular clusters. Now if the outer parts of the sidereal disc, removed from us by only the radius of the disc, present so faint an appearance to us as has been described above, how inconceivable would be the faintness of the whole disc when removed to a distance exceeding its own diameter many hundreds of times. It would not only not be resolvable into discrete stars by any telescope yet constructed, but it would be absolutely invisible in a telescope exceeding the Parsonstown reflector a hundred-fold in power.

I do not lay any stress on the above reasoning, founded as it is on suppositions which I am not disposed to admit. But imperfect as it is, it forces us to accept one of two conclusions, both of which are diametrically opposed to received opinions. We must either admit that the outer parts of our galaxy differ wholly in constitution from the parts which lie in our neighbourhood, or we must deny that the assumed external clusters bear the slightest resemblance to our own sidereal system.

I turn, however, to the examination of the stellar and nebular systems on principles more closely according with observed appearances.

We have seen that the elder Herschel was mistaken in supposing that the system of nebulæ forms a zone resembling the Milky Way, but nearly at right angles to its course. Had this been the case, a negative argument of some force might have been deduced in favour of the absolute independence of the stellar and nebular systems. For it is clear that if nebulæ really form a system of island-universes, and our galaxy be but a member of this system, it would be antecedently improbable that the apparent distribution of nebulæ should exhibit any correspondence whatever with the apparent distribution of stars. To take a perfectly parallel case:—The Sun is undoubtedly a member of the sidereal system, and we see that there is no correspondence, nor any approach to correspondence, between the position of the Milky Way (or the apparent zone of stellar aggregation) and the position of that medial plane near which all the members of the solar system are observed to travel. If there *had* been any correspondence of this sort, it must have been looked upon either as a very singular coincidence, or as evidence of some law of association into the nature of which astronomers would have set themselves to inquire. Now I can see no reason for anticipating that any association should exist between the position of a system of universes and the position of the galactic disc which is assumed to be a member of that system: nay, I can assert with absolute certainty (on the assumption implied) that no such association *necessarily* exists—since the spiral and elliptic nebulæ, which, by the assumption, are members of that system of universes, exhibit every variety of position. Therefore, had there been any correspondence of the kind conceived—that is, had there been a zone of nebulæ nearly coincident in position with the zone of the Milky Way—we should have had no other resource but to explain that correspondence as the effect of a very singular coincidence—unless we gave up the theory that the nebulæ do, indeed, form a system of universes whereof the galaxy is but a member.

Now I have been particular in dwelling on this point, because the discovery that Herschel had been mistaken as to the existence of a zone of nebulæ brought with it a remarkable result. We have seen that the central region of the northern cluster of nebulæ lies very near the pole of the Milky Way—so near, indeed, that the younger Herschel places this coincidence of position amongst the phenomena which any one who attempts to give a consistent theory of the nebular system must account for. We see, then, that there *is* a coincidence, not precisely of the kind contemplated in the preceding paragraph, but marked enough in its character. And we see also that Sir John Herschel—apparently without noticing the important conclusion to which his words tend—points out that we cannot reasonably ascribe this coincidence to the effects of chance-distribution, but must assign a cause for it. If Herschel is right, if accident is *not* a legitimate explanation of this coincidence, then, as it seems to me, we can adopt no other conclusion than this—that nebulæ do not form a system of external universes, but are intimately associated with the sidereal system.

But we have seen [1] that there exist in reality an association between the apparent distribution of nebulæ and stars, which is much more remarkable than the one pointed out by Sir John Herschel. For, along that very zone of the heavens which is occupied by the Milky Way there is a marked *absence* of nebulæ. If we except certain star-clusters, which exhibit so singular a relation to the Milky Way that Sir John Herschel considers they must belong to it, we shall find that of 6,000 and more nebulæ which have been discovered by astronomers, there are scarcely fifty which lie on a zone occupying a full tenth part of the celestial sphere. It will be remembered also that the direction of the central line of this zone is not exactly coincident with that of the Milky Way, but lies more nearly along the great circle through Orion, Perseus, Cygnus, Lyra, &c., near to which so large a proportion of the more brilliant stars are found collected.

Now the existence of a zone in which nebulæ are markedly wanting is a much more remarkable phenomenon than the existence of a zone very rich in nebulæ would have been. We see, for instance, that the existence of the Milky Way among the stars is very easily accounted for by the supposition that the sidereal system forms a species of disc. But if there were a zone wanting in stars, how should we have explained so strange a phenomenon? It is clear that we should either have to assume the existence of *two* sidereal systems, between which our Sun was situate; or else to adopt the almost equally *bizarre* theory that the stars form a cylindrical system, very long in comparison with its thickness, so that from the neighbourhood of the Sun—placed somewhere near the axis of the system—very few stars could be seen in directions at right angles to that axis. And, if the zone were very distinct, as is the case with the zone free from nebulæ, the former hypothesis would alone be available.

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[1] See preceding Essay.

It results, then, that if nebulæ really belong to external space, they must form two systems, our own galaxy occupying a place between the two. Improbable as this conclusion appears, we cannot escape from it, nor from this further improbability, that the sidereal disc should have a position almost exactly at right angles to the line joining the central parts of the two nebular systems—*unless we concede that the nebulæ belong, for the most part, to our galactic system.* Nor does there seem any comparison between the difficulties involved in the latter view and the utter improbability of either of the two former assumptions.

But again, let us recall the results of the discussion respecting the extinction of light. We saw that there is good reason for rejecting the theory that light suffers appreciable extinction within the limits of our galaxy. It will be well, however, before proceeding further, to notice that the theory thus rejected is not only not unfavourable to the views I am seeking to maintain, but leads directly to their establishment. For if we assume with Struve that the outer parts of the Milky Way are hidden from our view, even with Herschel's eighteen-inch reflector, through the effects of extinction, then, *à fortiori*, all *outer* systems must be hidden from us, unless their component stars exceed the brightest members of our system many thousand-fold in splendour. We see, then, that the theory of extinction at once excludes all belief that the nebulæ are external sidereal systems resembling or in any sense comparable with our own.

Now it has been assumed, somewhat too hastily I think, that the only available explanation of the difficulties which induced Struve to accept the theory of the extinction of light, lies in the supposition that the stars composing the sidereal system are much less densely strewn round its borders than in the neighbourhood of the Sun. It appears to me that, although in all probability there is a gradual diminution in the density of stellar aggregation as the distance from the centre of the galaxy increases, yet it is highly probable that the outer stars are inferior also in magnitude and splendour[1]—and perhaps in a much more marked degree. To me, indeed, the evidence in favour of such inferiority appears altogether irresistible.

In the first place, let us consider the naked-eye aspect of the Via Lactea. Is it such as would be presented if the sidereal system really has the figure assigned to it by Sir Wm. Herschel? I am not here discussing the first assumption made by Herschel,—that there exists a certain approach to uniformity in the distribution of stars throughout the galaxy. He was the first to admit that this idea must be abandoned. But it appears to me that Herschel's estimate of the irregularity of our galaxy fell far short of the reality. The sidereal system must be looked upon as composed of streams and sprays and clusters of stars aggregated together without any as yet recognised laws. The appearance of the Milky Way to the naked eye strongly suggests a constitution of this sort. It is commonly asserted that the galaxy forms a continuous ring of light upon the heavens. But this is not the case.

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[1] Sir John Herschel speaks of certain stars seen in parts of the galaxy as appearing small, 'not by reason of excessive distance, but of a real inferiority in size or brightness.' But he is not here supporting the theory we have mentioned above. I am speaking of the inferiority of the outer stars on the average, as compared with the average of stars near the centre of the galaxy; Herschel is speaking of the inferiority of certain members of the outer parts of the galaxy as compared with their neighbours.

Near Argo the main stream is divided, across one of its widest and most brilliant portions, by a dark rift of considerable extent. The narrower stream which runs side by side with a portion of the main stream of the Milky Way is also discontinuous; again, the galaxy is marked in some places by *lacunæ* and in others by sudden accessions of splendour—phenomena which appear inexplicable unless we assume an irregularity, not merely in the distribution of the stars, but in the form and structure of the sidereal disc.

But this is not all. Even on the assumption of extreme irregularity, there still remain insurmountable objections to the supposition that the Milky Way has the figure of a disc. It will be remembered that there exists in the southern hemisphere, close by the Southern Cross, a strange, roughly circular, or more correctly a pear-shaped vacancy, so conspicuous that it has obtained among sailors the name of the *Coalsack.* It is very difficult to reconcile the existence of a gap of this figure with the imagined longitudinal extension of the galactic system. And it is equally difficult to account for the phenomena described in the following passage of Herschel's 'Astronomy':—From the neighbourhood of Eta Argûs the Milky Way 'crosses the hind feet of the Centaur, forming a curious and *sharply defined* semicircular concavity of small radius, and enters the Cross by a *very bright neck or isthmus* of not more than three or four degrees in breadth, being the narrowest portion of the Milky Way. After this *it immediately expands* into a broad and bright mass.' Still more remarkable and significant is the fact that the Coalsack lies 'in the midst of this bright mass.' All these phenomena point to the conclusion that the Milky Way, in this neighbourhood at any rate, is really what it appears to be,—a belt or zone of stars, separated from us by a comparatively starless interval. An irregular belt of this sort might present the varieties of form indicated above, and might be transpierced by an aperture of any figure; but it is utterly inconceivable that a disc-like space, irregularly occupied by streams and clusters of stars, should be so transpierced as to exhibit a circular vacancy to an eye placed near its centre.

In the conclusions just deduced I am in agreement with Sir John Herschel, who, indeed, says in one place that the galaxy, looked at according to a certain view, would 'come to be considered as a flat ring.' But he nowhere adopts the consequences to which, as it seems to me, this view of the subject should have led him. In the interpretation of another phenomenon presented by the Milky Way I find myself wholly at issue with him. He says, 'We cannot without obvious improbability refuse to admit that the long lateral off-sets which at so many places quit the main stream and run out to great distances, are either planes seen edgewise, or the convexities of curved surfaces viewed tangentially, rather than cylindrical or columnar excrescences bristling up obliquely from the general level.' To me the obvious improbability seems to lie altogether the other way. That *one* plane or curved surface should happen to be seen edgewise, so as to resemble a long and narrow lateral off-set, is sufficiently unlikely; that several should so appear is utterly improbable. The obvious improbability conceived by Herschel appears to be founded on the 'oblique bristling up' of cylindrical star-sprays. But the evidence we have already had of the extremely irregular and heterogeneous conformation of the Milky Way is sufficient to remove any difficulty of this sort. Indeed, even among the stars in our immediate neighbourhood there exist, as I have pointed out in the first essay of this series, decisive evidences of stream-formation. The star-streams visible to the naked eye are far too marked to be the result of chance-distribution; and I anticipate confidently that the examination of the proper motion of all the stars composing any stream will suffice to show how intimately they are associated together.

Now it might seem, at first sight, that the phenomena I have adduced in the preceding paragraphs afford no evidence in favour of the opinion that the more distant portions of the galaxy are composed of stars inferior in brilliancy to those which lie in the neighbourhood of the solar system. But there is another phenomenon which, when properly understood, seems to make this opinion the direct corollary of the views resulting from the former phenomena.

It appears to me that, if we consider the enormous distance at which the Milky Way must lie beyond the lucid stars, on the hypothesis that it is composed of orbs nearly equalling them in brilliancy, we cannot but contemplate with amazement the singular correspondence which may be traced between the configuration of the Milky Way and the arrangement of the brilliant fixed stars in its neighbourhood. There is scarcely a part of the Milky Way in which there is not some evidence, more or less marked, of a much more intimate association between the lucid stars and the clustering orbs which constitute the galaxy than could be reasonably looked for as the result of chance-distribution. Consider, for instance, that bright light-cloud which adorns the noble cross in Cygnus, and the equally marked galactic clustering near Aquila: and note that this arrangement becomes the more significant when we remark that the two clusters lie on different streams of the Milky Way, which is double in this part of its course. Well worth noticing also is the conformation of the galaxy where it traverses the festoon of Perseus. But in the southern hemisphere there is a much more marked agreement between the Via Lactea and the lucid orbs. It cannot be merely the result of accident that the double curve of bright stars which forms the body and tail of Scorpio should so closely follow a most complicated portion of the Milky Way, that not one of those stars should fall on a part of the heavens free from milky light. Nor can it be accidental that the numerous turns and windings of the Milky Way from Antares to Sirius should seem in every case to be the result of attractive influences exerted by the leading stars in its neighbourhood, insomuch that there is not a single star of the first four magnitudes on any of the numerous lacunæ which appear in this portion of the Milky Way.

If we accept the evidence afforded by this peculiarity, we must suppose that the streams of stars composing the Milky Way are very much nearer to us than they have been assumed to be, and therefore that they are composed of stars far inferior in brightness, and also much more closely compacted, than those in the neighbourhood of the solar system.

The evidence afforded by the telescopic aspect of the galaxy strikingly confirms this conclusion. I shall mention two phenomena alone, as space will not permit me to deal with this part of my subject at any great length. The first is a peculiarity which is utterly inexplicable by the views ordinarily held—the fact, namely, that in several instances the Milky Way is observed to commence quite suddenly, so that one-half of the telescopic field of view will be occupied by nebulous light or by closely aggregated stars, while the other will be perfectly black—the line of demarcation between the two portions being well defined. The second phenomenon to which I wish to call attention is yet more significant. In some instances there is seen in the field of view a sharply defined projection from the Milky Way, the apex of the projection being occupied by a lucid star. That such an association should be looked on as accidental is exceedingly unlikely.

Corresponding to the association between the Milky Way and lucid stars is a phenomenon which falls here to be considered. The irregular nebulæ are objects differing altogether in character from all other nebulæ. They cover a far larger space on the celestial vault, even if we assume that their discovered dimensions afford any but the roughest indication of their real extent. In reality, however, since each increase of telescopic power increases the apparent dimensions of these objects, we probably fall far short of the truth in making such an assumption. Now as we have seen in the preceding paper these irregular nebulæ are associated in the most singular manner with fixed stars in the same field of view. The wisps and sprays of nebulous light which stream from the central convolutions of such nebulæ, correspond, quite closely in many instances, with streams of small fixed stars. In many of these nebulæ, also, there are streams of faint nebulosity extending towards fixed stars, and acquiring a sudden brightness around them. Now it seems to me that we cannot without utter improbability consider such an arrangement as accidental. For instance, if the bright stars ε and ι Orionis were wholly disconnected with the great nebula in Orion, how enormous would be the antecedent improbability that these orbs should appear—as they do—involved in strong nebulosity, connected by streams of faint nebulosity with the great nebula. And even if we assumed this to be possibly due to the effects of chance-distribution, how should we explain the fact that similar phenomena are observed in the other irregular nebulæ, and notably in that mysterious object which surrounds Eta Argûs, the most remarkable variable in the heavens.

Then, also, there is the significant fact that all the irregular nebulæ fall on that very zone which is freest from ordinary nebulæ.[1] And the only one which, though falling on this zone, does not actually fall on the Milky Way, is the

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[1] There is a notable exception to this law in the singular nebula 30 Doradûs, *which occurs within the greater Magellanic Cloud.* This exception is more sig-

Orion nebula. (See, however, Plate VI.) This last fact is even more significant than the general association of the irregular nebulæ with the Milky Way. For it exhibits the brilliant array of stars in Canis Major and Orion, with which the Orion nebula is associated (*and around certain members of which it actually clings*), as more intimately associated with the neighbouring stream of the Milky Way than the ordinarily accepted views would allow us to suppose.

I pass over the association often observable between those remarkable objects the double nebulæ and double stars, though the phenomenon is sufficiently significant. But there is one other well-established phenomenon which deserves attentive consideration. Nebulæ have been observed to vary in light, or even to disappear. Hind, d'Arrest, and Schmidt record many such instances. Are we to suppose that whole galaxies of suns have suffered in this manner total or partial extinction? Such a supposition is absolutely incredible. Nor can I look on the alternative that some opaque or semi-opaque substance has intervened between us and these objects as having any reasonable claim to acceptance.

The phenomena I have been discussing seem to point to conclusions very different from those which have been usually accepted respecting the visible universe. Instead of separating the stars and nebulæ into distinct systems, or rather of looking on the stellar system as a member of the system of nebulæ, we seem compelled to look on almost every object

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nificant, if possible, than the law itself, as will appear farther on. It is rather singular that Sir John Herschel should speak of 30 Doradûs as 'unique *even* in the system to which it belongs,' as if it differed yet more markedly from objects not belonging to the Nubeculæ, whereas, so far as one can judge from his own description and pictures, this nebula presents a striking resemblance (in its general character) to that which surrounds the star Eta Argûs. In each there is a mass of irregular nebulosity surrounding a central condensation, within which is a well-defined opening free from nebulous light; and the brightest star within the confines of each nebula occurs on the brightest part of the nebula and close to the vacancy.

visible even in the most powerful telescope as a portion of one system, which comprises within its range single, multiple, and clustering stars, irresolvable nebulæ, gaseous bodies of symmetrical and unsymmetrical figure, and in all probability myriads of other forms of matter as yet undetected. It would be rash indeed to attempt to speculate on the processes by which the visible universe has attained its present figure. But I may venture so far as to point to the evidences which seem afforded of processes of aggregation, leading—according to the position, and perhaps of the character of the masses acted upon—to the formation of suns of greater or less splendour and magnitude, of streams and clusters of small stars, and of systems in which suns and stellar streams and clusters seem to be intermingled. These processes seem to have led to an annular or spiral, rather than to a disc-shaped galaxy; but large portions of the matter, originally distributed perhaps with comparative uniformity, appear to have escaped the influence of these processes. Either because they have been subjected to counteracting attractions, or through the influence of the same principle which makes the centrifugal force near the poles of a rotating globe less than that at the globe's equator, this portion of the universe seems to have been free to form aggregations in regions which lie near to what may be called the polar axis of the galaxy. Nor need we wonder that these aggregations should differ very much in character from those which prevail within the galactic annulus, nor that within the former regions alone true nebulæ should be found profusely distributed.

The only irregular nebulæ which have been examined with the spectroscope—the Orion and Argo nebulæ—shine with light whose source is mainly, if not wholly, gaseous. Lord Rosse states that the stars visible in the Orion nebula, when examined with his giant reflector, appear as red points of light upon a bluish-green background of nebulous light; and it is possible, or rather, probable, that these points of light proceed from bodies which are not gaseous. But, however this may be, it is quite clear that there is in the Orion nebula an enormous amount of gaseous matter, forming (it would seem) a connected but irregular system, within which are involved many fixed stars, and notably the second magnitude star ε Orionis, and the third magnitude star ι Orionis. It seems fairly presumable that the other irregular nebulæ consist in like manner of enormous aggregations of the same luminous gases. The annular and planetary nebulæ appear to be, without exception, gaseous masses. Now, we have seen that all the irregular nebulæ lie within, or close to, the Milky Way. The same is the case with the annular nebulæ, and by far the larger number of the planetary nebulæ.[1] The Dumb-bell nebula, one of the most remarkable gaseous nebulæ in the heavens, also lies on the Milky Way. We see, then, that the gaseous masses revealed to us by the telescope show a marked tendency to aggregate along the galactic zone. *Why* this should be the case, it would not be easy—in the present state of our knowledge—to determine; but it is clearly not a phenomenon which need surprise us when once we have accepted the conclusion that stars and nebulæ form but a single system. I do not consider that, in extra-galactic space, the luminous gas which constitutes the common material of all the gaseous nebulæ (for the spectroscope reveals no variety in this respect[2]) is in reality wanting. It probably exists, but in a more dispersed form than in the galaxy. It is not a little remarkable that the only comets yet examined with the spectroscope exhibit (as respects the light from the nucleus) a spectrum indicative of gaseity, though not the same three lines of light which form the spectrum of the gaseous nebulæ. Is it not possible that around some stars, or systems of stars, there are gaseous bodies in some degree resembling comets? If we suppose that such an arrangement exists, we should be able to understand the permanence of the apparent figures of these objects, since the slow motions of cometic bodies in the enormous orbits indicated by our hypothesis would not be appreciable even in hundreds of years.

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[1] Of thirty-four planetary nebulæ recorded in the 'General Catalogue,' no less than twenty-one lie within 15° of the great circle centrally dividing the Via Lactea.

[2] Some of the gaseous nebulæ give a spectrum having one bright line; others give a three-line spectrum; and there is one nebula the spectrum of which consists of four lines. But it is presumable that these variations result only from variations in the intensity of the light of these nebulæ, since the bright lines occupy always the same position.

I cannot but think that there is some significance in the circumstance that so many temporary stars[1] have made their appearance 'in or close upon the borders of the Milky Way, and,' as the younger Herschel says, 'only within the following semicircle, the preceding circle having offered no example of the kind.' May there not be a connection between this peculiarity and the circumstance that so many of the more remarkable variables lie near the Milky Way? I have already noted the association of Eta Argûs with a large irregular nebula. Betelgeux, in the neighbourhood of the Orion nebula, is another remarkable variable. Near the nebular region of Cygnus there are also several variable stars.

The Magellanic Clouds remain to be briefly considered. Two arguments have been made use of to show that these mysterious objects are not connected with the galactic system:—First, they contain forms of nebulæ not met with within the Milky Way; and, secondly, there are no traces of any streams of nebulous light leading from the Milky

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[1] Sir John Herschel says, 'all, *without exception*;' but, since this was written, the temporary star which appeared in Corona in May 1866, has formed an exception to the rule we have referred to. It is probable that so-called temporary stars are in reality merely variables of long period and fitful variability.

Way towards the Nubeculæ. The former argument presents no difficulty. It is, indeed, rather a confirmation of my views that they afford an easy explanation of what had been held to be a scarcely explicable phenomenon. That the processes of aggregation in portions of space not falling within the galactic annulus should, in certain regions, lead to the exhibition of forms seen within that region, can hardly be considered very wonderful. But, in connection with the second argument, there is a circumstance which deserves to be carefully attended to. Herschel dwells forcibly on the exceeding barrenness of the regions which surround the Nubeculæ. 'The access to the Nubecula Minor on all sides is through a desert,' he says, in one place; and among his notes on this district we find such expressions as 'a miserably poor and barren region,' 'a region of utter barrenness,' and so on. Now, this peculiarity, so far from confirming Herschel's opinion that the Nubeculæ are disconnected with the sidereal system, is directly opposed to it. One can understand the phenomenon, if one looks on the Nubeculæ as aggregations formed within regions of space belonging to the sidereal system—one would almost *expect* that the neighbourhood of such regions should be deficient in splendour—*drained of stars*, so to speak. But, if the Nubeculæ were really distinct systems far beyond the sidereal system, there could be no reason for expecting that their neighbourhood should be more barren than other portions of the sky—still less that it should be *oppressively barren*. May we not go farther, and say that there is no way of accounting for so remarkable a phenomenon, save on some such hypothesis as I have presented?

But this is not all. It has been well remarked by Sir John Herschel, that the Nubeculæ are so nearly circular as to render the assumption that they are otherwise than globular in figure utterly improbable. It follows, therefore, that the farthest part of either globe is not much farther off proportionately than the nearest part. Hence the Nubeculæ show us that 'stars of the seventh and eighth magnitude and irresolvable nebulæ may coexist within limits of distance not differing more in proportion than as nine to ten.' Surely this circumstance is of greater force than Sir John Herschel seems to assume. He says that 'it must inspire some degree of caution in accepting *as certain*' the views ordinarily held respecting stars and nebulæ. To me the fact that stars and irresolvable nebulæ appear intermixed in the Nubeculæ seems to afford decisive evidence of the justice of the views which I have been induced to accept on other grounds. In the face of such evidence, the old theories respecting the universe seem to become wholly untenable.

From the *Student* for February, March, and April 1869.

WHAT FILLS THE STAR-DEPTHS?

For more than two centuries and a half astronomers have studied the depths of heaven with the telescope, piercing farther and yet farther into wondrous abysms of space, gathering clearer and yet clearer information as to the structure of celestial objects, and accumulating an untold wealth of knowledge respecting the habitudes of the great system whereof our sun is a constituent orb. During all this process of research the great end and aim of astronomers has been to extend the range of their instrumental appliances, in order to analyse more scrutinisingly the features of each portion of the celestial depths. Now and then it has occurred to some among their number to endeavour to combine the results which have been gathered together with so much pains; but these attempts have been almost lost sight of amidst the continual accumulation of fresh facts. The efforts made to arrange and systematise our knowledge have been altogether out of proportion with its extent.

And very strangely, when any attempts are made to educe from the labours of observers their proper significance, to reap the harvest which is already ripe, or rather to grind the corn which is already in our garners, the cry is raised that such attempts are fit only for the theorists, that they argue a want of appreciation of the labours of observers, and that we have more to hope from fresh observations than from any process of mere reasoning. Surprising, indeed! that those who say 'Let us use the observations already made' should be accused of undervaluing observation; and that those who can find no value or significance in past observations should call so eagerly for fresh ones!

I make these remarks because I am about to exhibit certain views respecting the habitudes of interstellar space, which have been formed from the study of the past labours of astronomers. I am fully sensible of the fact that to many I should seem better worthy of a hearing if I nightly timed my watch by the stars, if I had spent a few years of labour in attempting to divide well-known double stars with inadequate telescopic power, or if I had in some other equally convincing manner exhibited my title to be regarded as a member of the now large array of amateur telescopists who work so hard and effect so little and suppose themselves to be practical astronomers. Let me not be misunderstood, however. It is only because I wish to see amateur telescopists engaged on more useful researches, because I wish to see them devote a little more consideration than they do now to the thought of advancing astronomy, that I speak slightingly of the modes in which at present they are for the most part wasting time. We want all their help, and more, to advance the interest of our well-loved science; all their telescopic appliances are too few for the work astronomers would like to see them doing.

In studying the heavens, we have always this great difficulty, that we are looking at objects which lie in reality at very different distances, but which appear to lie on the concave surface of a vast spherical enclosure. It seems almost hopeless to attempt by any processes of observation to obtain reliable estimates of the distances of all, save a very few, of the fixed stars. It is not going too far to say that we are tolerably certain of the distance of only one star in the heavens—the star Alpha Centauri. This being the case, and the heavens spangled with millions of objects at altogether unknown distances, we must look carefully round us for evidence of another kind than that derived from actual measurement, we must look for signs of association, for definite laws of aggregation—if any such exist—and, if possible, we must apply that mode of inquiry from analogy which Sir William Herschel found in many instances so effective.

And here, as I have mentioned the name of this great astronomer, to whom we owe the first systematic survey of the heavens, and the first attempt to reduce the results of observation into law and order, I wish, with extreme diffidence, to point to what I cannot but consider an error of judgment in his selection of the principles which were to guide his survey of the heavens. It appears to me, that it would have been in all respects better had his first processes of stellar observation been directed to gauge the probability that this or that law of distribution prevails in the heavens, rather than to the application of a system of star-gauging, which, if founded on a mistaken assumption was necessarily but a waste of labour. It would have been a misfortune if the unequalled observing qualities of either the elder or the younger Herschel had been misapplied for a single hour; but the possibility that the labours of both these astronomers should have been devoted year after year to a process which (if my views are just) was practically useless, is painful indeed to reflect upon. It is true that the labours of the Herschels have been so numerous and so widely extended, that even the recognition of their star-gaugings as of little real utility would leave the great mass of useful results credited to them almost unaffected; but it would remain none the less a misfortune that labours, which in the case of other men would have worthily filled a lifetime, should have been misdirected.

And yet, when one considers the matter apart from preconceived notions, how inconceivably small the chance appears that those laws of distribution believed in by Sir William Herschel actually prevail within the sidereal depths. How amazing that to his clear perceptions the idea should ever have seemed probable that the celestial spaces are occupied only by orbs resembling our Sun! For be it distinctly noted, that his belief in the existence of gaseous nebulæ, and orbs in various stages of development, belongs to the later part of his career as an observer. Undoubtedly the whole system of star-gauging was founded upon the belief that the sidereal system consists of stars, varying greatly perhaps in size, but still not so greatly but that the least of them would be visible in Herschel's great telescope, as far as the very limits of the sidereal system, and that these stars are distributed with a certain general uniformity throughout space.

It is well to observe how fatally any error in this fundamental hypothesis affects the significance of any system of star-gauging. We turn a telescope in a given direction, and we see, perhaps, but four or five faint stars. According to the Herschelian hypothesis, the limits of the sidereal system are near to us in that direction, because the stars seen are so few; and these stars being necessarily within those limits, and faint, belong probably to the lower orders of real magnitude. But what if that hypothesis be erroneous—if there may exist in this or that direction vast blank spaces a thousandfold larger, perhaps, than the whole sphere of the visible stars in extent? Then, perchance, these four or five faint stars may lie farther from us than the farthest belonging to some of the richer star-fields; they may form a group of orbs which individually surpass Sirius or Canopus in magnificence, and are separated from each other by distances exceeding many thousandfold those which separate our Sun from neighbouring luminaries. But yet again, suppose that in any direction our telescope reveal crowded star-fields, orbs of all orders of apparent brightness, 'strewn as by handfuls, and both hands full,' and each increase of power adding fresh riches to the display. According to the Herschelian hypothesis, there is but one explanation of these wonders; we are looking into a widely extended part of the sidereal system, and those different orders of stars lie at different orders of distance—the farthest at distances so enormous that we cannot attain to them. But in what a different light we must regard the scene if we remember the possibility that that wondrous wealth of stellar display need by no means argue enormous extension! All those sparkling orbs may be gathered into one region of space, their various orders of apparent lustre arguing various orders of real magnitude. Instead of looking into star-lit paths, which extend linearly from the eye far out into space beyond the ordinary limits of distance separating from us the outer bounds of the sidereal system, we may in fact be contemplating a wondrously variegated star-group.

But the conclusions we are to form must be founded not on the consideration of what *may* be, but on our observation of what *is*. There is abundant evidence for forming probable views respecting the general laws prevailing within the sidereal system,—at any rate for deciding whether it is more probable there is or not any general uniformity of distribution within its limits.

One direct consequence of the laws of probability has been very much lost sight of in dealing with the subject we are now engaged upon. It has been urged that where so many stars are spread over the heavens, at so many various distances, we ought not to be surprised if very great varieties of distribution should be observed, nor conclude, therefore, that the general uniformity predicated by Sir William Herschel may not prevail as respects distribution in space. It has been forgotten that the vastness of the numbers in question should tend to a uniformity of apparent distribution, instead of the reverse.

I had been led myself to overlook this consideration, obvious as it is, until it was impressed upon me in a very striking manner during a somewhat novel process of research.

I wished to determine what peculiarities of distribution might be expected to appear among a number of points spread over a plane surface perfectly at random. It is clear that this is a preliminary consideration very necessary for the purpose of determining whether the laws of distribution seen among the stars are accidental or not. Now, the problem of determining by purely mathematical considerations what peculiarities would probably appear in a chance distribution of any given number of points, is one which may be regarded as altogether too difficult for solution. Very simple problems of probability have been found perplexing, insomuch that two eminent mathematicians of the last century are said to have disputed over the question whether the chance of tossing one 'head' and one 'tail' in two throws of a coin were one-half or one-third.[1] But problems concerning the chance distribution of points are specially difficult, as any one will find who tries a few apparently simple ones.[2] Therefore, I sought to solve this particularly complex problem in a practical manner, by simply spreading a number of points at random, and examining the result. But *how* to distribute points perfectly at random? It seems very easy, but is not so by any means. Suppose we take a handful of grains, and throw them upon a table. Will they then be strewn without law or order? Very far from it. The fact that they have all come from the same hand will lead to very obvious effects, taking away altogether from the desired random character of the distribution. Then again, suppose we were to distribute grains over a table from a sieve as large in extent as the table, and uniformly filled. In this case the grains would be distributed with a uniformity not appertaining to chance distribution. And so of a number of other contrivances which may be thought of; in every case of mechanical distribution, we always find either an enforced inequality or an enforced equality of distribution, not that really random distribution which we require.

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[1] The erroneous reasoning by which the answer is made to be one-third seldom fails to puzzle the uninitiated. 'There are,' said D'Alembert, 'three possible events: either two heads must be thrown, or two tails, or head and tail; of these three possible events, only one is favourable. The chance of that event is, therefore, precisely the same as the chance of drawing one particular ball out of a bag containing three—that is, it is one-third.'

[2] For instance, here are two: (1.) On a square surface of given size (say one square foot) two points are marked in at random; what is the chance that they will be within a given distance (say one inch) of each other? (2.) Three bullets strike a circular target three feet in diameter; what is the chance that the lines joining the three points where the target is struck will include a triangle less than one square foot in area?

The plan I actually adopted, if laborious, was at least satisfactory in this respect. I took a table of logarithms (any other book full of tabulated figures would have done equally well), and, opening the book at random, brought down the point of a pencil upon the page of figures. The numeral on which, or nearest to which, the point fell, I entered in a book. In this way I took out several thousand figures, following each other in altogether random sequence. Then, having divided two adjacent sides of a square into 100 equal parts, I drew parallels to the sides, through the points of division, thus dividing the square into 10,000 small squares. Now suppose the first four figures in my list to have been 73 | 24. I took the *seventy-third* parallel measured from one side, and the *twenty-fourth* measured from the adjacent side of the square, and at the point where these lines intersected I placed a black dot. I treated the next four numerals in the same way; and so on until I had exhausted the series. I thus had upwards of 1,000 dots distributed perfectly at random over the square.

Now, as I went on marking in the dots, I found that at first groups and streams might very well be imagined to exist among the dots. But as the process continued these groups and streams were obliterated (so to speak), until at length, when all the dots were marked in, it required a very fanciful imagination indeed to conceive that any signs of special laws of distribution existed among them. I was thus reminded of the great law of probability, that the mere numerical increase of trials ensures a steady increase in the uniformity of the results. For example, if one tosses a coin a few times, there will often result a very remarkable preponderance of 'heads' or 'tails;' but when one continues tossing the coin a great number of times, the ratio between the number of 'heads' and 'tails' approaches more and more nearly to equality. And, applying this law to the case under consideration, it follows that if a very large square sheet were divided into an indefinitely large number of small squares, and an indefinitely large number of perfectly equal dots were marked in according to my plan, or according to any plan securing a perfectly random distribution,[1] an accurate miniature of that sheet (taken by photography, suppose) would be found as uniformly tinted by these chance-distributed dots as by any mechanical process of uniform dotting.

Therefore, supposing that any general approach to uniformity of distribution exists among the stars, we ought to find all signs of special arrangement disappearing as we extend the range of our researches. We cannot then possibly explain the peculiarities actually observed as due to the enormous number of stars and the resulting probability that remarkable arrangements might accordingly be looked for. The exact reverse is the case.

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[1] One of the most interesting results of any such process as that above described, is the striking evidence afforded of the fact that any circumstance affecting the random character of the distribution is sure to tell when many trials are made. I was led to inquire whether in my list of numerals any special number seemed unduly to preponderate. I found that the number 8 appeared oftener than the rest, and that to an extent which I could not ascribe to mere accident; 1 and 7, on the other hand, appeared less frequently than the rest. The reason is obvious: the figure 8 covers more space, 1 and 7 less space, than any other figures; so that when the point of the pencil fell between an 8 and one of these figures, the chances were more favourable to the 8 being selected as the figure nearest to which the point came.

Now I conceive that so soon as we pass the third or fourth orders of star magnitude, we reach orders large enough, numerically, to supply the information, clear of the effects of mere accident, which we actually need in this instance. Among the stars down to the fifth magnitude, there is surely a sufficient number to enable us to begin to reason, with some degree of confidence, as to the constitution of stellar space. Therefore when, in 1866, I was constructing my gnomonic maps of the heavens, in which stars of these orders are included, I was disposed to regard the signs I met with of special laws of distribution as significant of real laws; and accordingly I put forward, in that year, the theory that the stars are aggregated into streams and clustering aggregations, with relatively bare spaces all round them. And, furthermore, it seemed to me, even at that stage of the inquiry into the habitudes of stellar space, that the Milky Way probably consists of relatively minute stars, and not, as had been supposed, of stars generally comparable with our Sun, and forming a system extending to enormous distances on all sides of us; while I was led to regard the nebulæ as belonging to the sidereal system, and not as external galaxies resembling that system.

But recently I have had occasion to apply processes of mapping to stars down to the sixth magnitude, or, in all, to four times as many stars as before. And clearly one cannot regard signs of arrangement among so many as 6,000 stars as being due to accident. The largeness of the number altogether precludes the possibility of this being the case.

When, therefore, it appears that among stars of the first six magnitudes there are signs of special laws of aggregation, we are bound to accept as legitimately following from the evidence, the conclusion that real laws of aggregation exist among the stars. We may not be able to tell what these laws are—we may mistake a number of separate clusters for a stream of stars, or the nearer end of a stream for the farther end, and so on; but the broad fact remains that the stars are gathered into some regions and withdrawn from others, and, further, that within the same region of space stars of very different orders are, in many instances, gathered together.

The general results of a systematic survey of the stars of the first six magnitudes seem certainly to force upon us such conclusions. They are as follows:—

1. The southern hemisphere contains more stars of the orders considered than the northern, in the proportion of about seven to five.

2. The stars of these orders are gathered into two definite regions—a northern and a southern—so markedly, that the distribution of stars within these regions is richer than the distribution over the rest of the heavens, in the proportion of about five to two.

3. The stars of these orders are associated in the most intimate manner with the Milky Way, insomuch that when the Milky Way is included with the two rich regions above named, it appears that stars in the single division thus formed are distributed about three times as richly as over the remaining portion of the heavens.

These results cannot be regarded as due to mere chance-distribution, unless we are to forget all the rules which the science of probabilities lays down for our guidance in such cases.[1] And if once we admit that they result from real laws of aggregation, our estimate of the nature of the sidereal system is wholly altered. We see at once that we are not dealing with a system that can be gauged; for if within the limits of naked-eye vision there exist these aggregations and these *lacunæ*, we may be full sure that throughout the sidereal system they exist also; and what confidence can we have in any system of gauging applied to depths so diversely occupied? Our sounding-line may light on a rich stream or clustering aggregation of stars, or it may pass through relatively vacant spaces, yet we can by no means conclude from the richness of the one region or the poverty of the other, that the line reaches the limits of the sidereal scheme either very far off or relatively very near.

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[1] The effect of numbers in diminishing the probability of such signs of law resulting from mere chance-distribution, must here again be insisted upon. It

Again, regarding either of the rich regions referred to above as consisting wholly or in part of a definite aggregation of stars happening to lie relatively near to us, is it not abundantly evident that other such aggregations at different orders of distance would exhibit many of the features which we see in the nebulæ, and have been in the habit of associating with regions lying beyond the sidereal system? Or—to arrive at a similar conclusion from different evidence—if the Milky Way be really, as it seems (from the third of the above results), a stream of stars of many different orders, with an enormous preponderance of relatively small stars, is it not abundantly evident that, supposing any portion of this stream removed to a greater distance, the fainter regions

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is most important to notice how it bears upon the conclusions we are to form. Here is a simple illustration of the law of probabilities in question.—Suppose we toss a coin 4, 8, 12, and 16 times, and inquire what is the chance that in the several cases either heads or tails will preponderate in the proportion of 3 to 1. Now the exact mathematical solution of this problem shows that when 4 trials take place, the chance is $\frac{5}{8}$, or more than $\frac{1}{2}$; when 8, the chance is $\frac{37}{128}$, or less than $\frac{1}{3}$; when 12, the chance is $\frac{299}{2048}$, or little more than $\frac{1}{7}$; while when 16 trials take place, the chance is reduced to $\frac{2517}{32768}$, or less than $\frac{1}{13}$. The chance would become indefinitely small if, instead of 16, we took several thousand trials.

would vanish first, and that the brighter regions would appear as small discrete patches of nebulous light,—or, in fact, as naked-eye nebulæ? Now, conceiving the stream yet farther removed, until even these patches become telescopic objects, would they not in all respects resemble the stellar but irresolvable nebulæ?

Thus it appears to me that we have not only no grounds for believing that the nebulæ are external galaxies, but tolerably distinct evidence that the stellar nebulæ are distant aggregations of stars of many orders of magnitude. Such aggregations may also themselves present, and doubtless they do present, all orders of magnitude, precisely as within the two rich stellar regions above referred to we find every conceivable variety of aggregation, and precisely also as within the Milky Way we find, on the one hand, bright regions as extensive as that which lies in the constellation Cygnus, and, on the other hand, regions as limited as the double cluster in Perseus.

Have we, on the other hand, any satisfactory reasons for regarding the nebulæ as external galaxies? Tracing back the course of that process of discussion which has led to the nebulæ being commonly so regarded, can we indicate any one argument which may be looked upon as definitely pointing to such a conclusion? *I know not of one.* I have carefully studied the writings of Sir William Herschel, and I venture to assert, without fear of valid contradiction, that every single consideration adduced in favour of the nebulæ being external galaxies has been founded on the assumption that the sidereal system is continuous—that is, on an assumption which Sir William Herschel himself was the first to throw doubt upon.

But the evidence derivable even from Sir William Herschel's own writings goes farther than this. He not only formed views respecting the sidereal system diametrically opposed to those which he had entertained when his conceptions respecting the stellar nebulæ were put forward, but he arrived, by his careful study of the nebular system, at a conclusion which, rightly interpreted, brings the nebulæ into close association with the sidereal system. For, noticing the aggregation of nebulæ at the northern region, which lies farthest from the Milky Way, he confidently expressed his belief that any sound theory of the universe must account for that peculiar relation. In other words, no theory of the universe can be regarded as sound which treats that relation as accidental. So that Sir William Herschel himself regarded the nebular system in a light which in effect associates it in a real manner with the sidereal system.

In fact, and in conclusion, that great astronomer was not bound, as so many of his modern followers have been, by his own earlier theories. As his researches continued, his views gradually changed. The process of change went on during the whole course of his career as an observer. He advanced steadily from truth to truth, and when at length the close of his labours approached, he looked onwards, not backwards. He would have been the last to desire that astronomers should take even his latest and best theories as including all that they could desire or hope to know; but it would have been even more painful to him to imagine that the views he held, when as yet his labours were but beginning, should be adopted by future astronomers in preference to those which were the fruits of his ripened experience.

From the *Popular Science Review* for July 1870.

STAR-DRIFT.

Of all the discoveries effected by Sir W. Herschel, none gave greater evidence of his skill in dealing with observed facts than his discovery that the solar system is sweeping onwards with enormous velocity through intersidereal space. The problem had been attacked a year or two before by an eminent German astronomer without success. Mayer had, indeed, announced definitively that the stellar motions afford no evidence to countenance the view that our sun is speeding through space. No other evidence lay before Herschel than Mayer had possessed, nor was there any flaw in Mayer's mode of reasoning. Undoubtedly the full evidence which Herschel had to deal with was unfavourable to the idea of solar motion. But no one knew better than Sir William Herschel that in questions of this sort old Hesiod's proverb is applicable, that 'Half is often more than the whole.' By throwing aside half the evidence, though that evidence already seemed sufficiently meagre, he deduced a result which all the exact and recondite processes of recent inquiry have scarcely been able to improve upon. He pointed to a certain region among the stars as that towards which our Sun is travelling, and around that region all the best determinations of modern times have ranged themselves. In Plates IX. and X. I have laid down all the points which Mädler, and Airy, and Argelander, and Struve have successively indicated as the apex of the Sun's path in space. These points cover a region of the heavens some twenty degrees in length and some ten degrees in width; but the last and best determination, the result of a new process invented by the Astronomer Royal, and applied (under the able superintendence of Mr. Dunkin, of the Greenwich Observatory) to no less than 1,167 stars, lies within three degrees of the spot which Herschel pointed to nearly a century ago.

It is easy to recognise the justice of the principle on which Sir William Herschel proceeded; and the subject is so intimately associated with that which I am about to discuss, that a brief sketch of his mode of dealing with the solar motion will not be out of place here.

The only evidence we can have respecting the movements of the Sun is that to be derived from the apparent motions of the objects which surround him. There is no irregularity in his stately progress through space to impress upon us who move with him the fact that he is not at rest. But the stars which lie on all sides around his path, must be affected with apparent motions unless they travel with him, not only in the same direction, but at equal speed. To the unaided eye no signs of stellar motion are apparent. There are not, indeed, ten stars in the heavens whose motion in a thousand years would cover an arc that the naked eye could estimate. But the skilfully constructed instruments in use in modern observatories enable the astronomer to measure even the seemingly evanescent movements of the so-called fixed stars. In ten years, or in twenty, no change of position may be apparent; but when the observations of our day come to be compared with those which were made a hundred years ago, the traces of stellar motion become in many instances unmistakeable.

But here one point is to be noticed. A star's change of place, even in long intervals of time, is often so minute that instrumental errors may mask it. Instrumental errors may do worse, however: they may unduly magnify the star's proper motion, or seem to give proper motion to a star which in reality has none. An illustration will serve to give the reader some idea of the delicacy of observation required for the determination of stellar movements. Every one knows the seven bright stars of the Great Bear. They form a group called by some the Butcher's Cleaver, by others Charles's Wain. Now, if any one looks carefully at the middle star of the bear's tail, he will notice very close to it a tiny star.[1] The distance separating the bright star from its companion seems very small indeed; yet a star would be thought to have an unusually large proper motion which should traverse in a hundred years one-tenth of this tiny space.

The result of this is, that the less perfect our observations are, the larger do the stellar motions appear to be. A careless observation at one or other end of the long interval of time made use of, will cause a star to simulate a proper motion altogether disproportionate to its real movement on the celestial sphere. I am not theorising, be it understood, on this point. The fact has been well established. I think it was Argelander who remarked that the best proof of the value of a new catalogue of stars is the extent to which it reduces former estimates of the stellar proper motions. I have a proof of this immediately at my hand. I open the pages of the celebrated 'Star Catalogue of the British Association,' and, running my eye down a column of proper motions, I select the ten largest. I find that out of the ten stars thus conspicuous for large proper motions, no less than seven belong to the southern heavens, or, in other words, to the part of the celestial sphere which has been least carefully surveyed. The inference is obvious.

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[1] The Arabs called this star 'the rider,' and the power of discerning it was among them a recognised test of sharpness of vision; but now it is very easily seen.

Now, Sir William Herschel felt that if he made use of assigned proper motions which were in reality due to carelessness of observation, the result would be vitiated. He saw that to multiply the number of stars he considered was to multiply the chances of an erroneous determination. Nay, if the proper motions of ten stars could be trusted, and a hundred doubtful proper motions were dealt with at the same time, it was all but certain that no satisfactory result could be obtained. Mayer's failure served to prove this.

What Herschel did then was to select only those proper motions which by their exceptional magnitude were safe from the influence of observational errors. To a few of these he trusted, paying no attention whatever to doubtful proper motions. The result was that he succeeded where Mayer, after much more labour, had signally failed. Like a master-bowman Herschel seemed to aim carelessly at the mark, yet struck it fair and full.

But even as Herschel saw the great truth he sought to establish shining clearly forth from amidst the gloom in which it had hitherto been enshrouded, he became conscious of other great truths which stood beside it. With two of these we are now principally concerned; and though as yet science has done little to present either of them in their just proportions, yet we know enough already to believe that they will hold a noble place in the science of future ages.

Our Sun, sweeping ever onwards through space, tells us of a great law of motion to which his fellow-suns also must be subject. He is but one among many, when viewed in relation to the galactic system. Nay, it is doubtful even whether among the suns which shine upon us from beyond the vast domain over which our own Sun bears sway, there be not many which as far surpass him in magnitude as the giant members of the solar scheme—Uranus and Neptune, Saturn and Jupiter—surpass our tiny Earth and its fellows among the minor planets. Nor is there aught in the Sun's position to lead us to ascribe to him alone swift motion throughout space. That, as *we* view the galaxy, he seems to occupy a central position is true enough; but it is conceivable, if not certain, that there is not a single sun among all the stars which deck our skies, from which the same sort of scene is not visible as that which is presented to the inhabitants of our Earth.

And this is not all. The very evidence Sir William Herschel had made use of proved that our Sun is not the sole moving star of the galaxy. Had he been able, by assigning a certain direction to the Sun's motion, to have accounted for the motions of all the stars his processes dealt with, he might then indeed have inferred the possibility that those stars at least are at rest. But this was not the case. The direction he assigned to the Sun's motion was that which accounted best for the stars' motions, not that which explained them fully. The balance of motion which remained unaccounted for could be explained only by supposing that the stars Herschel had considered were themselves sweeping rapidly through space. This view was directly involved in the statements Sir William Herschel laid before the scientific world.

This was one of the great truths associated with Herschel's noble researches; and it is the one with which we are here most immediately concerned. But there was another which must be considered before we proceed to examine the evidence we have respecting the nature of the stars' proper motion, and particularly the evidence which justifies my description of the sidereal movements as star-drift.

If the Sun is moving now in a definite direction, it can by no means be inferred that this motion will always continue to be directed towards the same region of space. All the analogies which surround us teach us to believe rather that his path is of the nature of a gigantic curve re-entering into itself mayhap like the planetary orbits, or possibly of a complex figure, resembling the paths of those comets which belong indeed to the solar scheme, but are swayed continually into new orbits by the attractions of the larger planets. Whichever of these views is correct, it is certain that the part of his path which the Sun is at present describing, must be looked upon as a portion of a gigantic circle. For, no matter what the figure of an orbit may be, any small portion of the curve may always be regarded as belonging to some definite circle. And astronomers have set themselves to inquire into the nature of the vast circle on which, for present purposes, we are to regard the Sun as travelling.

The ingenious German astronomer Mädler, by a process of inquiry into which I need not here enter, has been led to regard the star Alcyone, the 'brilliant' of the Pleiades, as the true centre of the Sun's motion. Without pretending to render the principle of his researches perfectly obvious to the general reader, I may yet briefly state how he has deduced a conclusion of so much importance.

He argues that if the galaxy has a centre within the range of the visible stars, a certain peculiarity must mark the motions of the stars which lie nearer to the centre than our Sun does. As has been already mentioned, the neighbourhood of the centre of a stellar system is a scene of comparative rest. In the solar system we see the planets travelling faster and faster, the nearer they are to the great ruling centre of the scheme; and the reason is very obvious. The nearer a body is to a great centre of attraction like the Sun, the greater is the attraction to which it is subject, and the more rapid must its motion be to enable it to maintain itself, so to speak, against the increased attraction; but in a vast scheme of stars tolerably uniform in magnitude and distribution, the outside of the scheme is the region of greatest attraction, for there the mass of all the stars is operative in one general direction. As we leave the outskirts of the scheme, the attraction towards the centre becomes counterbalanced by attractions towards the circumference; and at the centre itself there is a perfect balance of force, so that a body placed there would remain in absolute rest. It is clear, then, that the nearer a body is to the centre, the more slowly it will move. This being so, all stars which lie nearer than the Sun to the centre must move more slowly than the Sun; and therefore, whatever the direction of their motion, they must *lag behind* as seen from the Sun!

So that if we can detect, in any part of the heavens, a community of motion, in a direction opposite to that of the Sun's motion, we may take it for granted (on the assumptions involved in Mädler's argument) that the centre of the sidereal scheme lies in that part of the heavens.

Now Mädler noticed that, in the constellation Taurus, there is such a community of motion as he had described; and carefully estimating the amount of motion observed among the stars of different orders of magnitude, he thought he saw reason for assigning to Alcyone a condition of absolute rest.

Here then, if his assumptions were correct, we were to recognise the central orb of the sidereal scheme; not indeed, the central orb in the sense which our Sun is the central orb of the solar scheme; not a globe which, by its magnitude, could be held to sway the motions of all the stars which lie around it. Alcyone, according to Mädler's theory, is simply the star which occupies the centre of the sidereal scheme, but without rule or governance over its fellow-stars.

These speculations of Mädler's form, I believe, the sole attempt which has yet been made to co-ordinate the motions of the stars into one systematic whole. They have not been looked upon as successful, nor has the consideration of the subject led astronomers to believe that the problem Mädler attacked is one which *can* be mastered in the present state of our knowledge.

The objections which have been urged against Mädler's speculations are too numerous and important to be overlooked. In the first place, his assumption that there is a central orb within the range of the visible stars, is one which is at present wholly inadmissible. There is nothing in the appearance of the heavens to lead us to suppose that the centre of the sidereal scheme is in our neighbourhood (so to speak). Rather we should be led, as the celebrated German astronomer, Struve, long since pointed out, to regard the distance which separates the Sun from the faintest of the lucid stars as altogether evanescent in comparison with the Sun's displacement from the centre of the galaxy. Again, Mädler's mode of treating the question involved the supposition that all the stars travel in almost circular paths around the centre of the sidereal scheme, since any great eccentricity in their orbital motion would involve a total change in the character of their motion at definite distances from the centre. Then he assumed a knowledge, on our part, of the distances of stars of different degrees of brightness, which was altogether more exact than any we can really pretend to possess. These and other considerations have led all the most eminent of our modern astronomers to look upon Mädler's hypothesis as one which, in the present state of our knowledge, we have no right to look upon with favour.

To these arguments I am able to add another which has hitherto, I believe, escaped notice—the fact, namely, that the community of motion which Mädler noticed in Taurus, and which he supposed to be exceptional, is in reality a feature commonly to be met with among the stars, and is, in fact, simply an illustration of that peculiarity which I have ventured to designate by the name of 'star-drift.'

Let us return, however, to the stellar motions which the searching eye of Sir William Herschel had detected at an early stage of the inquiry.

As the apparent motions of the stars were examined with greater and greater care, it became more and more evident that every star is rushing swiftly through space; for, although every fresh determination of the nature of the Sun's motion showed a general agreement with Herschel's result, yet there was a sufficient divergence to prove that some causes other than variations in the method of calculation are at work in causing the discordance.

But it is only in comparatively recent times that it has been shown demonstratively, how largely the apparent motions of the stars are due to the real movements of these bodies in space. The Astronomer Royal commenced, in 1859, a process of inquiry which was at once more rigid and more complete than any which had hitherto been applied to the subject. The principle of the new mode of inquiry was remarkably simple. Mr. Airy argued that, if we are to obtain a really conclusive answer to the great question of the Sun's motion in space, we must remember that the stellar motions, though apparently taking place upon the celestial sphere, have really no relation at all to that imaginary surface. We must look upon them, he said, as taking place in reality in space, and apply to them the mathematical processes which result from that consideration. It need hardly be said, that, as we know nothing of the motions of recess or approach which stars may have—though the spectroscope, in the able hands of Dr. Huggins, promises soon to remove that defect in our knowledge—the whole question becomes one of probabilities. But we can deal mathematically with probabilities of this sort, and thus deduce from a number of observed relations, whose actual significance is uncertain, the result which is mathematically the most probable. This was the problem which Airy solved, and it cannot be doubted that the solution of a problem so complex and perplexing is one of the most useful results which modern astronomy has achieved.

With the solution of this problem, however, came several unlooked-for difficulties.

The point towards which the Sun was found to be moving, accorded most satisfactorily with the results which had been obtained by other methods. The rate assigned to the solar motion was also in close accordance with that which the younger Struve had obtained by a different process. I may mention in passing what this rate is, because it is important that we should have clear notions respecting the amount of motion which we may look for among the members of the sidereal system. It appears, then, from the researches of Otto Struve on the one part, and those of the Astronomer Royal and Mr. Dunkin on the other, that the Sun is moving at the rate of about a hundred and fifty millions of miles per annum. Although this velocity seems enormous, it is in reality smaller than the velocities we notice in many parts of the solar system. Our own Earth moves more than three times as fast in her orbit, as the Sun on his path through space. Indeed, the diameter of the Earth's orbit is thirty millions of miles greater than the distance annually traversed by the Sun.[1]

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[1] The peculiarity of the Earth's real motion in space, when her orbital motion is combined with the Sun's progressive motion, has proved a source of strange difficulty to many persons, ever since Sir William Herschel announced his solution of the problem of the Sun's motion. Although Newton had provided for the possibility of the Sun's motion, many imagine that the Newtonian theory of gravitation, and even the Copernican theory are endangered by the

So far the results of the Astronomer Royal's researches were in close accordance with the views already entertained by astronomers. But in the progress of the inquiry certain processes had been performed upon the tabulated proper motions of the stars which rendered it easy to apply an interesting test to the theory of the Sun's motion, or rather to the power which that theory may have of accounting for the stellar motions. It was possible in fact to apply the effect due to the Sun's motion in space, as a correction to the stellar movements. It seemed clear that if this were done, the result should be that the total amount of apparent motion would be largely diminished. If a person is moving rapidly amidst a crowd of moving persons, the appearance of movement in the crowd is in general much more striking than it becomes when he stands still; and what the Astronomer Royal was really doing—or rather what he was theoretically doing—was to make the stars' motions such as they would be if the Sun were suddenly reduced to rest.

But the result of these processes was not that which the Astronomer Royal had looked for. The stellar apparent motions, instead of being considerably reduced by the change, remained very nearly as large as they were before.

Speaking of this result nearly a year after the Astronomer Royal's labour had been published, Mr. Main, then President of the Royal Astronomical Society, remarked that 'the inevitable logical inference deducible from Mr. Airy's researches, is that the whole question of solar motion in space, so far, at least, as accounting for the proper motions

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complexities thus introduced into the solar system. It need hardly be said that the question of the path actually pursued by the Earth in space is merely one of curiosity, the orbital motions of the Earth being absolutely unaffected by the progressive motion which she shares with the Sun. As a matter of fact, it is worth mentioning that the Earth's path in space resembles the thread of a screw having a skew axis—the direction of the Sun's motion being inclined about fifty-four degrees to the plane of the Earth's orbit.

of the stars is concerned, appears to remain in doubt and abeyance.' It must be understood, however, that the doubts here expressed by no means rest upon the fact of the Sun's motion. We are forced to believe that *some* of the assumptions upon which we had been proceeding with confidence, are more than questionable. It may be, for example, that our ideas of the stellar distances require to be modified. Or again, it may be that the stars are not moving independently, but according to some system which tends to falsify all the general assumptions we had formed on the hypothesis that they are independent suns like our own. And, again, there may be laws of motion which associate the stellar movements in a special manner, without any actual association of the individual orbs.

I was led some time ago, by considerations wholly distinct from those which I have been dealing with above, to form the opinion that if the proper motions of the stars were *mapped*, there would be rendered apparent signs of association between stars much farther apart on the heavens than the members of the widest double or multiple star-systems. In the third essay of this series, I pointed to the fact that associated proper motions would afford significant evidence in favour of the theory that the signs of stellar aggregation in certain parts of the heavens are not accidental.

I therefore proceeded to map down the proper motions which the Astronomer Royal had used in his calculations. (See Plates IX. and X.) It need hardly be said that a map of proper motions speaks much more intelligibly and clearly respecting the meaning of those motions than the most carefully constructed table could do. Astronomical tables are necessarily arranged in such a way that stars which lie near each other in the heavens are often far apart in the catalogues. It is therefore impossible to form any clear conception of the general character of the motions prevailing in any given region of the heavens. When a chart had been so constructed as to exhibit the stellar motions to the eye, this difficulty was at once removed.

The plan I adopted was to attach to each star a little arrow, whose direction and length indicated the character and magnitude of the star's proper motion. In order that these arrows might not be too small, I gave them the length corresponding to the star's motion during a very long interval of time. For convenience, I made one degree on the map correspond to a stellar motion of one-tenth of a second; the result being that the motions actually represented are those which would take place in the course of 36,000 years. Even so magnified, most of the motion-arrows are inconveniently minute.

The maps included at first only the 1,167 stars dealt with by the Astronomer Royal, but I subsequently added upwards of 400 stars, whose proper motions had been calculated by Mr. Stone, of the Greenwich Observatory, from observations as trustworthy (having, in fact, been made by the same observers) as those which Mr. Main had used in preparing the catalogue of 1,167 stars.

The maps[1] fully justify the term 'star-drift,' which I have applied to the stellar proper motions.

Remembering that the stars which are visible to us lie at very different distances, we see that if a real star-group exists in space, having dimensions which cause it to appear to cover a widely-extended region of the heavens, we must expect to find that other stars in the same region do not belong to the group. Not only so, but it must be looked upon as highly probable that the stars seen in any given direction may belong to three, four, or more star-groups, if the existence of star-groups is a real fact. Therefore we might expect the existence of star-groups to be more or less masked by the effects which would follow from their apparent intermixture. If, for instance, a general concurrence of proper motions in a definite direction is to be held indicative of the fact that the stars so moving form a single system, then we might expect in general to fail in detecting such systematic drift, on account of the perplexities introduced by some other drift belonging to a set of stars apparently mixed up with the former. And if three or more star-drifts were mixed up together in this way, the problem would become still more perplexing.

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[1] The maps show also the estimated position of the apex of the solar way according to the researches of Sir W. Herschel, O. Struve, Argelander, Mädler, the Astronomer Royal, and others. Arrows have been placed over all parts of both maps, corresponding in length and direction to the estimated apparent motion of a star of the first magnitude, supposed to be at rest, but changing in apparent position on account of our Sun's motion. Thus it becomes possible to determine at once whether a star or set of stars in any region be moving, or not, in a way corresponding (in direction and rate) with the effects due to the Sun's motion.

Thus, all that was to be hoped for (as it seemed) was that here and there some sufficiently well marked cases of star-drift might not be masked by the effect of other motions.

The result, however, was more satisfactory than I had anticipated.

In some regions large groups of stars are seen to be drifting bodily in a definite direction. The most remarkable instance of this sort occurs in the stars which form the constellations Gemini and Cancer. All these, amounting in number to seventy or eighty, are drifting towards the neighbouring part of the Milky Way, with the exception of three stars, which seem to belong to another system. Another remarkable instance is to be found in the stars of the constellation Taurus. This is the instance of concurrent proper motions on which Mädler founded his theory that Alcyone is the central sun of our galaxy. The drift in this part of the heavens is in a manner opposed to that in Cancer and

Gemini. The two systems are, in fact, divided by the Milky Way, and each seems drifting towards that zone.

More commonly, however, two or three forms of star-drift are seen intermixed in the same region of the heavens. And here it might seem difficult to pronounce whether in reality there are any associated movements, since it would clearly not be impossible that a mere chance distribution of motions might simulate a tendency towards two or three definite directions. There is, however, a circumstance which at once serves to establish the true significance of the observed relations. If the motions in one *direction* have, besides, a general agreement in respect of *magnitude*, we can clearly assume with a much greater degree of probability that they indicate a real drift, or rather, that the stars they belong to form a real system. Nay, the mere fact that a number of stars in a given region have a very minute proper motion, while all the rest have large motions, would show that the former form a system perfectly distinct from the latter. One instance will serve to show the power of this new mode of discrimination.

Of the seven bright stars in the Great Bear, five are travelling in a common direction with uniform velocity. The other two are travelling in another direction and also with a common velocity. We cannot doubt that the first five, at any rate, form a system, drifting along bodily. For let us sum up the evidence. First, we have the comparatively weak evidence derived from the general equality of the five stars, a peculiarity which has in all ages led observant men to entertain the impression that these stars are in some way associated. Next, we have the fact that the five stars are travelling in the same apparent direction, and the significance of this point it is easy to estimate, because the antecedent probability that, taking the direction of one star of the five as a standard of reference, the other four would be found to be travelling in the same direction, is demonstrably minute. Lastly, we have the evidence derived from the equality of the motions of the five stars, and here again the antecedent probability of the coincidence is so minute as to force upon us the opinion that the actual coincidence is not accidental. The combination of the three lines of evidence leads to a feeling of absolute certainty that the five stars are associated into a single scheme or system.

Let us pause for a moment to contemplate the significance of this result. One of the stars of the set of five is the middle star in the tail, which I have already referred to as having a companion visible to the naked eye. Now, it had long since been proved that the bright star and its small companion are really connected. The evidence had been no other than that community of proper motion which I have been dealing with in the case of the five stars. Mizar and Alcor, then, were looked on as a wide double, and astronomers had contemplated with interest and amazement the wondrous cycle corresponding to the motions of stars separated in reality by an enormous interval. For the nearest of the stars in our northern skies is more than 720,000 times farther from us than we are from the Sun. Mizar is presumably much farther away. Now, whatever distance separates Mizar from us, cannot be more than about 240 times as great as that which separates Alcor from Mizar: so that Alcor must be at least 3,000 times farther from its primary orb than we are from the Sun. How enormous, then, must be the period required for the revolution of the two stars around their common centre of gravity!

But this is not all. Mizar has a close companion, as well as its distant companion, Alcor. This close companion has the same proper motion as Alcor and Mizar, and belongs, therefore, to the same scheme as the other four stars. A sort of dignity is thus given to the star-system we are considering, by the triplicity of one of its members. At present, however, we are dealing with another consideration —the magnitude, namely, of the cyclic period appertaining to the scheme. Now, the close companion of Mizar, though undoubtedly it is in reality travelling round that star, moves yet so slowly that no sign of a change of place has as yet been detected by astronomers. Therefore the period of revolution, even for this comparatively close pair, must be very large, and Alcor must have a very much longer period; so that we may accept without surprise Baron Mädler's estimate that Alcor occupies no less than 7,659 years in travelling around Mizar.

But what sort of periods can we assign to the cyclic revolutions of the five stars, when the comparatively close companions of one of the set occupy periods of revolution so enormous? Again, how can we resolve the questions which at once suggest themselves respecting the relations which prevail in such a system? That the whole system revolves around its centre of gravity is of course certain. But there are numberless ways in which the revolution may take place, depending on the relations between the weight and velocity of the different orbs forming the system. Any two of the five may really form a pair, any three may form a triplet. We cannot tell where the centre of gravity of the scheme may be. We have no knowledge of the true relative positions of the five orbs. We cannot guess what the real direction of their orbital motions may be. We are, in fact, altogether in doubt on every subject connected with the system, except the main fact that the whole system has a drift carrying it bodily forwards at the rate of many millions of miles per annum. It is in this connection that the appearance of such systems as these in the heavens, seems to me so interesting—I may almost say, so imposing a phenomenon. The life of man is a period too short to tell us anything even of the subordinate motions of such a scheme, —the motion of Mizar's companion about its primary, or of Alcor about both; but the duration of the human race, nay, of the solar system itself, may be out-lasted by a single revolution of the great star-system placed out yonder in the celestial depths. From the far-off times of the Chaldæan shepherds the great Septentrion star-system has looked down with seemingly unchanging aspect on the rise and fall of many nations and races of men. When the human race has perished from this globe, when the earth has become what the moon now is, a scene of utter barrenness and desolation, the star-system will doubtless have exhibited many changes. But only when millions of æons have passed, and the earth is nearing the scene of its final absorption beneath the solar oceans, will the stately motions of the star-system have begun to work out the full series of cyclic changes appertaining to a scheme so extensive and so complicated.

But the star-drift in Ursa Major is only one instance out of many. Looking more closely than we have yet done into the sidereal scheme of which our Sun is a member, we see it breaking up into subordinate star-systems of greater or less extent. Our Sun himself may not be a solitary star as has been commonly supposed. From among the orbs which deck our skies, there may be some which are our Sun's companions on his path through space, though countless ages perhaps must pass before the signs of such companionship will be rendered discernible. On every side we see drifting star-schemes, and comparatively few stars are to be recognised as voyaging in solitary state through space. From the complexity of such systems as we see in Gemini and Cancer, to schemes such as the one in Ursa Major, and thence to solitary stars such as Arcturus and Sirius appear to be, we recognise a number of gradations, and it yet remains to be determined to what class of these schemes our Sun belongs.

Verily much remains to be learnt respecting our galaxy. Since the days of Sir W. Herschel, or rather since the younger Herschel completed the noble series of labours commenced by his father, a sort of rest has fallen upon astronomy, so far as the science deals with the relations of the great sidereal system. But there is room for much new labour in this wide field of research. The Herschels dealt with generalities. They discussed the galaxy as a whole, and it was no part of their work to examine into the details of the stellar scheme. The work they took in hand to do they accomplished with marvellous success, insomuch that they have left little for others to achieve in the same direction. But it would be a mistake to renew the Herschelian mode of inquiry—to continue to neglect details and consider only the grander features of the galaxy. The work of survey has been completed. In examining, part by part, the field which has been plotted out for us, we must adopt new principles for our guidance. To deal, *now*, with generalities alone, as the Herschels did, would be to destroy those scarcely recognisable indications which can alone guide us to new knowledge. We must in future examine the sidereal scheme detail by detail, feature by feature. The work will not be light, and many workers will be wanted. But the result will be worth the toil. Not in our day, perhaps not for many generations, may the fruits of such labours be reaped. But gradually astronomy will gather in her harvest, and when it is garnered, the rich reward of many years of toil will be found in a clear knowledge of the relations presented by the wondrous galaxy to which our Sun belongs.

From the *Student* for October 1870.

ARE THERE ANY FIXED STARS?

During the last few years astronomers have been attacking questions which seem, at first sight, far beyond the range of the human intellect, or of the instrumental appliances which human ingenuity can devise. A marked contrast, indeed, is to be distinguished between the inquiries which have been made within the last decade and the most valuable discoveries of all previous times. Not one of the results which had rewarded the labours of scientific men up to the middle of the present century would have seemed incredible to Francis Bacon had it been predicted to him: nay, there is scarcely one of them which is not more or less distinctly shadowed forth in that strange and little read work of his, the 'Sylva Sylvanum.' But even he, daring as were his conceptions and hopeful as were his views of the powers of that method of research which he inculcated, would probably have smiled with contempt had the idea of analysing the Sun or the fixed stars been mooted in his presence. The Frenchman who lately brought before the Imperial Academy at Paris the absurd proposition that our astronomers and physicists should make signals to the inhabitants of Mars and Jupiter scarcely appears a greater dreamer to us than any one would have appeared to Bacon who put forward a notion seemingly so preposterous.

At first sight it may seem to many that the subject I have now chiefly to deal with—the determination, namely, by our astronomers, of the motions of recess or approach which the fixed stars may possess—does not belong to the category of those researches which appear altogether hopeless. Yet when the true nature of the problem is understood, a different view will certainly be adopted. It will be well to look at the subject of the stellar motions in this way, to consider the difficulties which seem to surround it on every side, and the interest which attaches to its solution, before we proceed to consider the method which has been successfully applied to one star, and will doubtless in the fulness of time be applied to hundreds, with results whose importance it is impossible to over-estimate.

The stars, it is well known, have to ordinary observers every appearance of fixity. If Hipparchus or Ptolemy could now look on the orbs they watched so lovingly in the far-off years, they would see nothing to induce them to imagine that the stars are in motion. Whether the aspect of the heavens is exactly the same now as when Aratus sang the glories of the constellations, we cannot indeed assert with any certainty of conviction. Many of the stars may shine with different lustre, some few have perhaps disappeared, and possibly some new stars have appeared upon the scene. But such changes as these are not in question at present; I refer only to apparent changes in the stars' places.

Astronomers in our day know indeed that the stars are changing their place upon the heavens. But it is not because the change of place has been made perceptible to ordinary vision; but because by means of the telescope it has become possible to magnify so largely the effects of change, that movements which would produce no perceptible effect in thousands of years if ordinary vision only were in question, are recognised as certainly as though the astronomer could see the star actually moving as he watched it.

But such changes of position as can thus be recognised are not only minute, insomuch that it is only after half a century of observation that even modern astronomy can detect them, but they afford no certain indication of real motion on the part of the star. *We* may be in motion—nay, more, it has been proved that we are in motion, that the Sun with his whole *cortége* of planets and cometary systems is sweeping swiftly through space, and the apparent motion of a star may in reality be wholly due to the Sun's motion. The very fact that by observing the apparent stellar motions astronomers have been able to guess the direction in which the Sun is travelling through space, shows that a large part at any rate of the stellar motions must be due to the Sun's motion. And inasmuch as we are by no means certain of the direction in which the Sun is moving, or of the velocity with which he rushes through space, we are not in any case able to determine how much or how little of a star's apparent motion is due to the solar proper motion. Furthermore, our uncertainty as to the distances of all save one or two stars renders us yet more doubtful how to interpret the stellar movements.

Still we may take it as proved by the mere determination of the stars' proper motions, that these orbs are not fixed in space. Because we are quite certain that no motion which could possibly be assigned to the Sun by astronomers would account for all the stellar motions, whatever assumption we might form respecting the stellar distances. That this is so will be evident from the simple consideration that there are cases where two stars near each other (in appearance) are moving in exactly opposite directions. We cannot possibly account for such motions as these by any assumption involving fixity for both stars and motion in the case of our Sun alone.

Furthermore, the motions of those double stars which form binary systems suffice to show that motion is an attribute appertaining to some stars; and if to some stars as well as to our Sun (which is but a star), why not to all?

But now notice a point of great importance in connection with the question of movements of recess or approach.

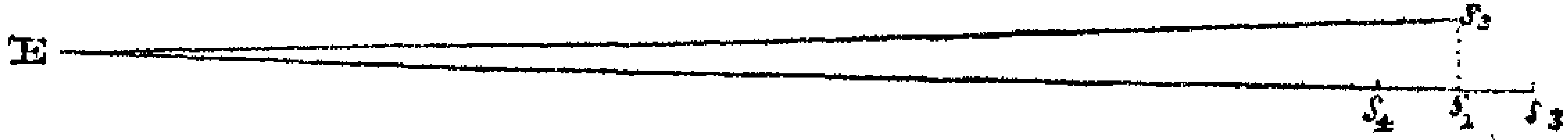
The apparent proper motions of the stars give us the means of estimating their probable motions of recess or approach, and thence of estimating our chance of determining such recessions or approaches.

In the first place it will be admitted that we have no reason whatever for believing the stellar motions to be limited to any special direction. If we imagine our sun for a moment set at rest, and that we could tell the exact direction in which each star moved, we should doubtless find that the stars were moving in every direction with respect to the lines of sight drawn to them. Here a star would be moving almost square to the line of sight; here nearly along it and towards us; here nearly along it and from us; and elsewhere every possible variety of direction would appear without the slightest preference (when the whole celestial sphere was considered) for one direction rather than another.

Amongst the immense number of stars, then, whose proper motions have been determined, there must be many whose motion is very nearly square to the line of sight from the observer on earth. And we have no reason for supposing that the stars which are thus moving have less or greater motions, on the average, than their fellows which are moving in other directions. Hence the motions of the former set of stars afford us a measure of the motions which those stars possess which are moving directly from or towards us, and so appear at rest; and though we can thus learn little respecting the real rate at which those stars are moving, for we know little respecting the real rate at which the other stars are moving square to the line of sight, we have a very satisfactory measure of the general rate at which the *relative* distances of the stars are diminishing or increasing.

For let E be the earth, s_1 the position of a star at the beginning of a century, s_2 its position at the end of a century, the star being one of those which is moving at right angles to the line of sight. And let us suppose that the star is also one of those which is moving most rapidly in appearance, so that the angle s_1Es_2 which measures the proper motion in a century is as large as possible. Then we cannot assume with any probability that any star in the heavens has a greater proportional motion of recess or of approach than the motion indicated by s_1s_3 or s_1s_4 (each equal to s_1s_2). That is, the ratio of Es_3 or Es_4 to Es_1 indicates the greatest relative change of distance we may look for among the stars during the course of a century.

Fig. 8.



But we can calculate the value of this ratio at once, and *so see at once what chance there is that a change in a star's brilliancy caused by such a change of distance could be estimated by means of our photometers.*

There is no star in the heavens which has so large a proper motion as ten seconds of arc per annum. Therefore in taking the angle s_1Es_2 at 1,000 seconds or 16′ 40″, we are taking a very favourable view of our prospect of estimating change of distance by change of brilliancy. If s_1Es_2 be an angle of 16′ 40″, then s_1s_2 is equal to 0·004848 where Es_1 is called 1. Put for convenience s_1s_3 or s_1s_4 as equal to ·005. Then the brilliancy of the star at the beginning of the century would bear to its brilliancy at the end (supposing there has been in the meantime no change in the actual amount of light emitted by the star) the ratio

$$\left(\frac{1,000}{995}\right)^2 \quad \text{or} \quad \left(\frac{1,000}{1,005}\right)^2$$

according as the star had approached or receded from the earth with the assumed (and altogether over-estimated) velocity referred to. These ratios reduce severally to

$$\frac{101}{100} \quad \text{and} \quad \frac{99}{100}$$

In other words, the star's brilliancy would only be increased or diminished by about one hundredth part in one hundred years even in this altogether exceptional case.

No instrument ever yet devised by man could give any indications of so slight a change of brilliancy as this, even if it occurred in a single instant, so that one and the same observer could measure the star's light under unchanged atmospherical conditions.

We see, then, that the problem of attempting to measure the motions of recess or approach which the stars may have is one which seems, on the face of it, as hopeless as that of determining what the stars are made of would have appeared twenty or thirty years ago.

Yet the problem has been mastered, and in a much more thorough manner than the seemingly simple problem of estimating the motion of the stars *athwart* the line of sight. In fact, whereas we know nothing (except in one or two cases) respecting the *amount* of this latter motion, for its angular measure affords no satisfactory criterion of the real motion in miles per year, we have a means of measuring the *real* velocity with which the stars are approaching towards us or receding from us. I proceed to show how this is done:—

We know that light is not a material emanation from luminous bodies, but consists in reality of the propagation of minute vibrations, taking place either in an ætherial medium pervading all space, or (as some physicists suppose) in the ultimate particles of what we ordinarily term matter.[1] Now it is to this property of light that the powerful mode of research which I am about to describe owes its effectiveness; and therefore it is necessary that we should attend somewhat closely to the peculiarities of wave-motion. Space will not permit me to enter at such length as I could wish into the considerations thus arising; and therefore I will confine my attention to a few primary points. The whole subject was dealt with in a highly interesting manner, I may remark, in the lecture delivered by Professor Miller to the working-men of Exeter, in 1868, the careful study of which will well repay the student.

The simplest illustration we have of wave-motion is in the material waves which traverse the surface of water. Now here, be it noticed, we must not think of such waves as roll in upon the shores of the sea, but of the true waves which traverse the surface of open seas. In such waves there is not, as there appears to be, a rapid transmission of matter, but a simple oscillation of the particles of the water. Suppose there are long rollers sweeping over the face of the ocean. Then we should call the distance from the crest of one roller to the crest of another the *wave length*; the difference of altitude between the crests of the rollers and the bottom of the 'trough' between them would be the *wave-height*, or *wave-amplitude* as it is sometimes termed: and the rate at which the rollers travel would be the *velocity of transmission*. I mention these points, so that in dealing with other waves which we are unable to recognise as visible entities, the significance of the terms we shall have to make use of may be understood by a reference to the familiar relations of the ocean-rollers.

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[1] It is often difficult in treating of the supposed ætherial medium to express oneself at once intelligibly and accurately. If there is an ætherial medium different in some essential properties from any of the substances we recognise as matter, yet that æther is still matter, and the laws which rule its movements are identical with those which govern the movements of other matter. By the term 'laws' I here signify, of course, only the general laws of motion—not special laws, as gravitation or the like. I am particular in dwelling on this point, because it appears to me to be too often forgotten. Physicists speak, for instance, sometimes of the possibility that light-waves may be propagated to an *infinite* distance from the source of light. This is simply an impossibility. That light travels, so far as it does without *appreciable* extinction, proves that the range of motion of the vibrating particles of the æther is very small indeed, compared with the wave-lengths; but this range, however small, must have a definite value at any given distance from the source, and the total amount of motion in *any* spherical surface round the source of light would have a definite and constant value. But to suppose a definite amount of oscillation in an infinite number of such spheres is to suppose that an infinite effect can accrue from a finite cause.

Now, suppose we wish to determine the wave-length of a rolling sea, what are the methods which would suggest themselves? If we could measure a line extending from crest to crest at any moment, we should, of course, know the wave-length; but failing (as might well happen) such a means as this, it is obvious that our resource must be to determine first the velocity of the waves, and secondly the number which pass in a given time. Suppose the first point gained (say by noticing the time which a particular roller occupies in travelling between two ships a mile apart), and, for convenience of illustration, let us imagine that the ascertained velocity of the rollers is 500 yards per minute. Now, suppose that an observer, counting the waves which pass the side of his ship, notices that 10 pass each minute. Then he knows at once that in that time the first which passed has travelled to a distance of 500 yards; and as all the 10 are distributed over that distance, each must be 50 yards in length, *if the ship has not moved in the interval*. But if the ship has moved, there is a difference. For supposing the ship to have moved in the same direction as the waves, and at the rate of 100 yards per minute, then the crest of the first wave is only 400 yards off instead of 500, and the wave-length is but 40 yards. In other words, the true length is less by 10 yards than the length which would be arrived at on the supposition that the ship is at rest. On the contrary, if the ship is moving at the same rate against the waves, the true distance of the first wave-crest is 600 yards, the true wave-length 60 yards; and the effect of the ship's motion is to cause an apparent increase of 10 yards in the wave-lengths.

And clearly, if we could only conceive such a state of things as that the ship should be really at rest and the whole mass of the rolling sea bodily transferred under the ship, we should get a similar result. If the transference were in the direction of the wave-motion (which would correspond to a motion of the ship against the direction of the waves), the result would be an under-estimate of the wave-length; while in the reverse case the wave-lengths would be over-estimated.

Now let us consider the case of sound-waves. These are somewhat less familiar to us (so far as our ordinary modes of perception are concerned); but, inasmuch as we can more readily make experiments on them than on light-waves (owing to the enormous velocity with which the latter travel), they will serve to give a convenient illustration of the property we are to deal with.

Let fig. 9 represent a series of sound-waves generated by the vibrations of the tuning-fork A. When the right-hand prong is at *a* (the limit of a vibration), *a* is a place of aërial condensation; the next such place is at *b* (*ab* being the *wave-length* corresponding to the vibrations of the tuning-fork), the next at *c*, the next at *d*, and so on. The *wave-amplitude* does not concern us, but I may mention in passing that it is measured by the degree of the aërial condensation at *a*, *b*, *c*, *d*, &c. The tone of the sound depends on the wave-length *ab*, and a given tuning-fork will cause aërial waves of a particular length (that is, will give out its proper tone), *if it be at rest.*

But now suppose that the tuning-fork is being moved, and that with a velocity bearing an appreciable relation to the velocity with which sound travels. It will readily be seen that the tone now produced by the tuning-fork will be different from what may be termed its natural tone.

Thus suppose that during the interval which sound would occupy in travelling from *a* to *b*, the tuning-fork has been moved so that the prong *a* is at *a′*. During the interval the prong has made one complete vibration, and *a′* is now therefore a region of condensation instead of *a*; *b* is, of course, a region of condensation, just as it would have been if the fork had been at rest. Hence the wave-length has been reduced to *a′b*; and as all the waves proceeding from the neighbourhood of the vibrating fork are similarly affected, there results a series of waves, *ef*, *fg*, *gh*, &c., as in fig. 10.

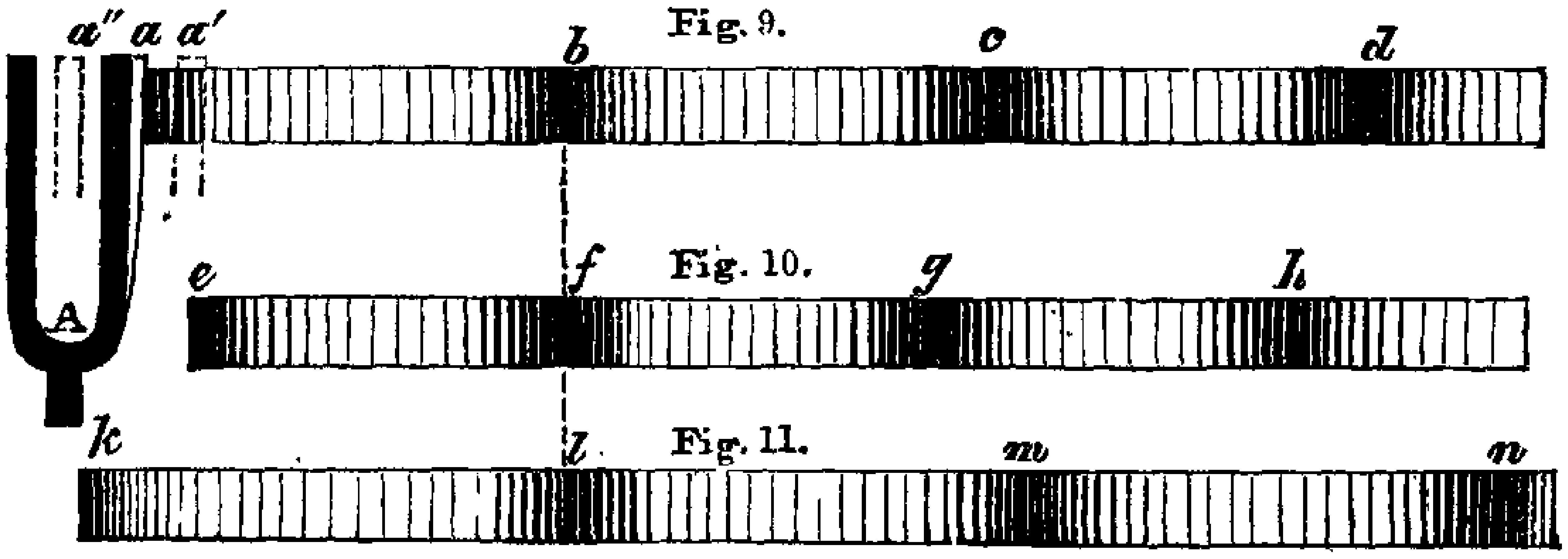
Fig. 9.
Fig. 10.
Fig. 11.

On the other hand, if the fork had been moved in the opposite direction, there would have resulted the series of waves *kl*, *lm*, *mn*, &c., represented in fig. 11.

In the former case the tone of the resulting sound would have been more acute, in the latter it would have been more grave, than the natural tone of the fork.[1] And a little consideration will show, that if, instead of the fork being moved, the ear were brought rapidly towards or from the vibrating fork, similar effects would follow—a rapid approach rendering the sound more acute, a rapid retreat rendering the sound more grave.

It is absolutely necessary, here, that the velocity either of the fork or of the ear should bear an appreciable proportion to the velocity of sound. In other words, *aa'* or *aa''* must bear an appreciable ratio to *ab*. This is obvious, since what is wanted is, that *ef* or *kl* should differ appreciably from *ab*.

Now, if we only suppose the vibrating end *a* of the fork to be a particle whose vibrations are generating light of a particular wave-length—that is, of a particular *colour*, we see that the reasoning we have applied to sound-waves must be equally true of these light-waves. If the source of light be approaching us, through its own motion, or ours, or both, the waves will seemingly be shortened ; and if the source of light be receding, the waves will be lengthened. In other words, there will be in either case a change of *colour*—the change being towards the blue end of the chromatic scale in the former case, and towards the red end in the latter.

But here, as in the case of sound, the condition has to be fulfilled, that the velocity of approach or recess shall bear an appreciable proportion to the velocity with which the waves travel; that is, to the velocity of light. Now, light travels at the rate of about 185,000 miles per second; and it seems hardly conceivable that any material movements in the universe should bear an appreciable relation to so enormous a velocity as this.

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[1] Professor Tyndall has remarked that when a train rushes rapidly past a station a change in the tone of the whistle may be noticed by a person on the platform, the sound being more acute as the train approaches than after it has passed the station.

We could not hope, then, that any luminous object in the universe should indicate by a change of colour a change in the direction of its motion.

But this is not the only nor the principal difficulty in the application of such a mode of estimating motion. Döppler, who was, I believe, the first to suggest that the colours of the stars may serve to indicate whether these bodies are approaching us or receding from us, omitted to notice a circumstance which rendered his whole argument nugatory:

In the case illustrated by figs. 9, 10, and 11, we dealt with the affections of only a single wave-length. If all stars sent us light rays having a definite wave-length, then what we have described would happen; and if our perception of colour were but sufficiently delicate, we could tell whether a star were moving from or towards us by the colour of its light. But this supposition implies that a star's colour should be monochromatic; and we know that the light from the stars consists of a combination of all the prismatic colours. But again, if the spectrum had definite extremities, and if no action *of any sort* took place beyond those extremities, then something like what Döppler conceived would take place. For then the light-waves of all lengths would be affected by a star's motion; so that if a star were approaching us, all the waves would be shortened, and a part of the red end of the spectrum would suffer extinction, while in the reverse case the blue end of the spectrum would be shortened. We know, however, that beyond the visible ends of the spectrum, waves too long and too short to affect the eye as light-waves are really in existence. Thus instead of the red end or the blue end suffering, in the cases imagined above, all that would happen would be that the heat-waves beyond the red end or the chemical rays beyond the blue end would become light waves, replacing the red or blue end of the spectrum, as the case might be: and there would be no change of colour.

But these considerations, while showing that nothing can be hoped for from Döppler's suggested consideration of star-colours, show that a much more delicate and satisfactory test can be applied. We see that the whole spectrum is shifted bodily. *Therefore all its lines, whether dark or bright, must be shifted with it.* This is a motion we *may* hope to estimate, because we can bring into comparison with any line in the shifted spectrum the corresponding line belonging to some terrestrial element. We have, in fact, a test of the most extreme delicacy; and were it not that the most rapid stellar motions can produce but the minutest change in the position of the star's spectrum, we might *read off* the stellar motions of recess or approach as readily as we can determine the general character of the star's light by the same mode of analysis.

But when it is remembered that the velocity of light is about 185,000 miles per second, we see that a star must be moving with enormous velocity that its spectrum may exhibit any appreciable change of position. Our sun is supposed to be travelling at the rate of about five miles per second, and we have reason to believe, from some researches of Mr. Stone's, that the average motions of the stars may be about one-third greater,—say about seven miles per second. Now, it has been estimated by Mr. J. Clark Maxwell that a velocity equal to that of the earth in her orbit, that is, rather more than eighteen miles per second, would shift the sodium line D_1 through a space equal to about the tenth part of that which separates D_1 from D_2, these lines forming what is commonly called the double line D of sodium. An idea, therefore, may be formed of the difficulty of estimating the stellar motions of recess or approach, unless in those exceptional cases where the star's real motion is much greater than the above-mentioned average.

Of course, the whole question is one of the dispersive power of the spectroscope; and inasmuch as a telescope of large aperture will permit us to use a higher dispersive power than we could apply to a smaller instrument, the size of our telescopes enters into this as into so many other questions of astronomical interest.

The star selected for the first application of the new method of research was Sirius, on account of its great brilliancy. It was necessary to consider some one recognised line of his spectrum, and the line corresponding to the solar line F (the blue-green hydrogen-line) was the one selected.

Fig. 12.

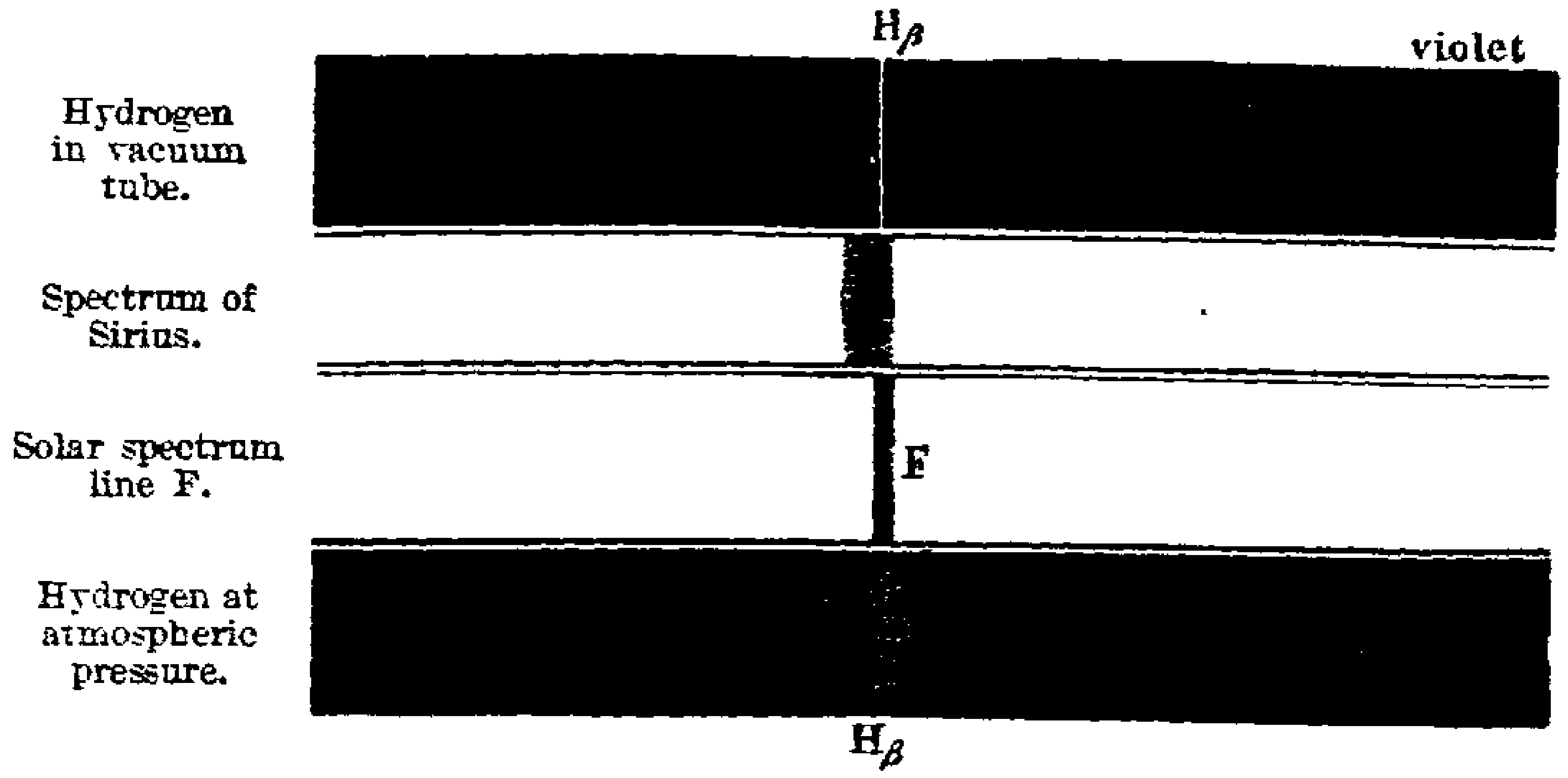


Fig. 12 shows the result of the experiment. The two upper spectra are not directly concerned in the method applied; but it is well to notice the perfect coincidence in position between the sharp dark line in the solar spectrum and the middle of the diffused line obtained from hydrogen at ordinary atmospheric pressure. Any want of coincidence *here* would have thrown doubt on the result of the experiment.

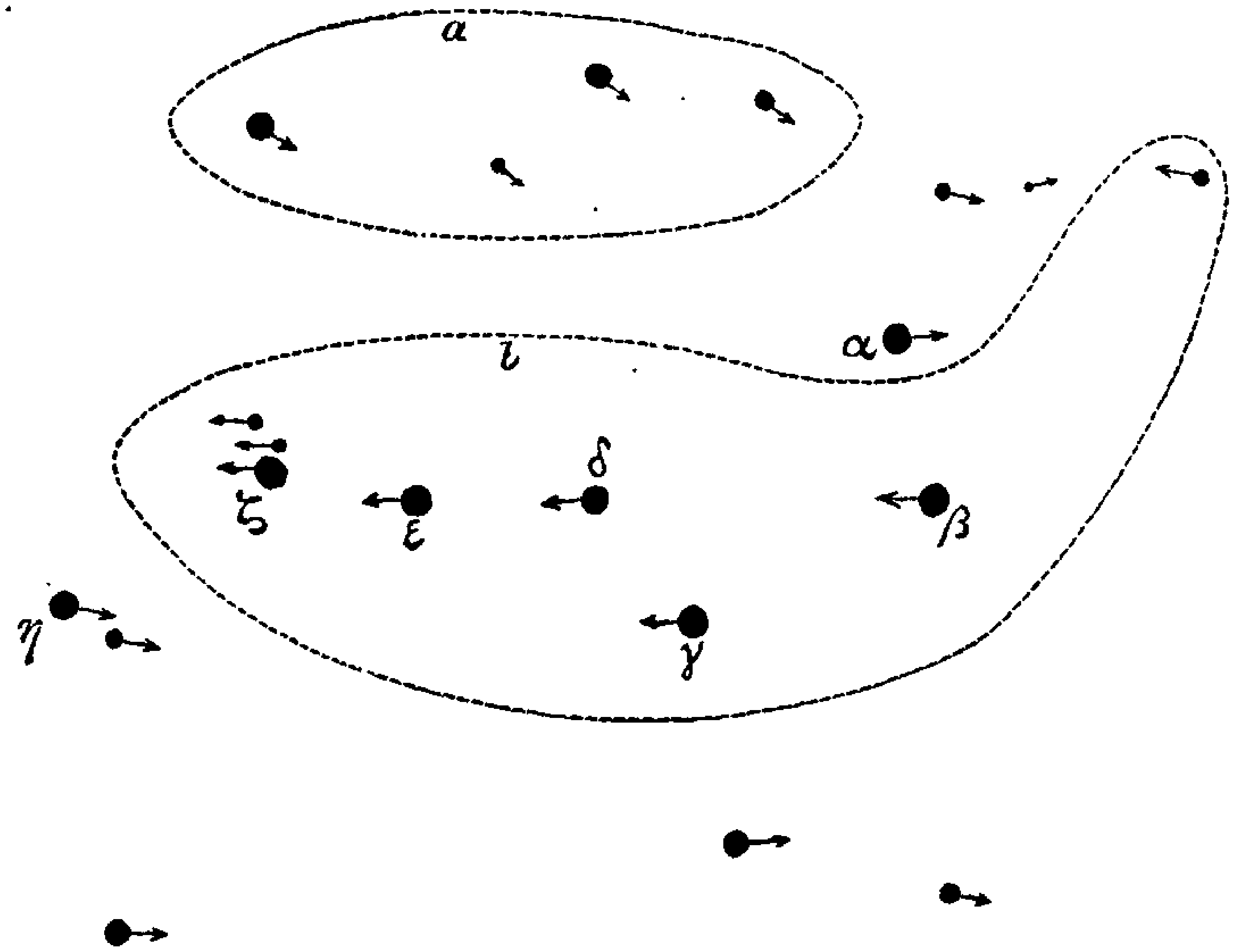
The hydrogen-line, actually compared with the dark and somewhat diffused F-line of the spectrum of Sirius, was obtained from hydrogen in the so-called vacuum-tube. Dr. Huggins made it fall side by side with the diffused dark line F in the spectrum of Sirius, and in some experiments he brought the bright line *upon* the Sirius-line. It will be seen from fig. 12 that the bright line fell sensibly away from the middle of the dark line. It became obvious from this that Sirius has a motion in the direction of the line of sight, and since the dark line was shifted towards the red end of the spectrum, it followed that the motion was one of recession.

From a careful measurement of the discordance between the two lower spectra of fig. 12, Dr. Huggins calculated that at the epoch of the observation Sirius was moving from the earth at the rate of 41·4 miles per second. But a part of this motion was due to the earth's motion in her orbit, and having made due reduction on account of this consideration, Dr. Huggins found that there remained a motion of recession *from the sun* of 29·4 miles per second. Lastly, he considered the effect to be ascribed to the sun's motion, which is directed towards a point almost exactly opposite Sirius. If Otto Struve's estimate of the solar velocity is correct, then the motion of Sirius *in the galaxy* is reduced to somewhat less than twenty-five miles per second.

Interesting as this result is, the fact that the power of the new mode of research has been established is yet more so; for there is nothing to prevent the method from being applied in turn to all the lucid stars; nay even, with suitable instrumental power, to the telescopic orbs. The results obtained from such researches cannot fail to be of the utmost value.

I take myself a special interest in the new method of research, because I hope to find its results confirmatory and elucidatory of certain peculiarities in the stellar motions which I have recently been led to notice. On mapping the proper motions of about 1,500 stars in the manner indicated in fig. 13, I have found in many parts of the heavens distinct traces of star-drift; that is, of the systematic motion of groups and sets of stars in particular directions. A singular instance of this is found among the bright stars of Ursa Major, five of which are moving (as shown within the oval, *b*, in fig. 13) in the same direction, and with the same velocity. Two other stars near ζ are also moving in the same manner. One cannot doubt that these stars are associated in some way, and so form a system; especially when it is noticed that the stars are moving in a direction almost directly opposite to that due to the sun's motion in space. Scarcely less remarkable is the community of motion observed within the oval *a*. And it will be noticed that among the remaining stars of the map there is a community of motion either *inter se*, or with the stars included within the ovals.

Fig. 13.



It will be a matter of extreme interest to determine by

Dr. Huggins's method whether the stars which thus seem to form drifting systems have a community of motion of recess or of approach.[1] Should this be the case, no doubt could possibly remain that the stars form sets or groups, and that there is no approach to that generally equable distribution described in our popular treatises of astronomy.

From the *Popular Science Review* for October 1868.

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[1] See next article for the confirmation of this prediction.

NEWS FROM THE STARS.

From time to time, during the last three years, I have brought before the students of science the various arguments and considerations on which I have based certain new views respecting the constitution of the sidereal universe. In so doing I have had occasion to deal chiefly with facts already known, though not hitherto viewed in that particular light in which I sought to place them. Indeed it is an essential part of my general argument that much that is contained in observations already made has been escaping us. In the eagerness of astronomers to ascertain new facts, they have been neglecting the interpretation of facts already ascertained.

But I have long felt that it would greatly tend to advance the new views which I have advocated, if some process of research, pursued by one of these astronomers of our day who possess the requisite means and leisure for prolonged inquiries, should confirm in a clear and decisive way some definite point of my new theories. Thus, if new observational evidence should be found in favour of my theory that the nebulæ are not external to our galaxy, or if new evidence should be obtained to show that the stars are aggregated in certain regions within our system and segregated from others; or again, if my theory of star-drift should be confirmed by new and striking evidence, I felt that a greater measure of confidence in my analysis of former evidence would thenceforward be accorded. I had no occasion, indeed, to complain of cavil or opposition; in fact, a degree of attention had been given to the new opinions I advocated which was certainly much greater than I had looked for. But there must always be such an inertia in the general weight of opinion in favour of accepted views, that only a steady reiteration of reasoning during a long period, or else some striking and impressive discovery, can cause the weight of opinion to tend in the contrary direction.

I cannot but regard myself as most fortunate in finding the first confirmation of my views (i.) coming from one of the most eminent astronomers and physicists of the day, (ii.) bearing upon one of the most definite and positive of my vaticinations, and (iii.) relating to one of the most interesting subjects in the whole range of recent astronomical research.

It will be in the remembrance of many of my readers that, nearly four years ago, Dr. Huggins succeeded in showing that the bright star Sirius is travelling at an enormously rapid rate away from us. In other words, besides that rapid thwart motion which is shifting the place of this star upon the heavens, the star has a rapid motion of recession. In the preceding paper, called 'Are there any Fixed Stars?' the nature of the means by which this discovery was effected is fully described and explained. It may be permitted to me to mention, also, that while Dr. Huggins's researches were still unannounced (or rather incomplete) I was so far fortunate as to indicate the possibility of employing the very method of research which Dr. Huggins was then engaged (unknown to me) in applying to Sirius. I propose here briefly to describe and explain the method, referring the reader who desires fuller information on these preliminary points to the preceding paper. I am the more desirous of doing this, because I find the principle of the method not readily grasped, and I conceive the explanation I am about to offer [1] may remove certain difficulties not uncommonly experienced.

Conceive that a person, standing on the edge of a steadily-flowing stream, throws corks into it at regular intervals—say one cork per second. These would float down the stream, remaining always separated by a constant distance. Thus, if the stream were flowing three feet per second, the corks would be a yard apart (supposing, for convenience of illustration, that each cork was thrown with exactly the same force and in exactly the same direction). Now, if a person a mile or so down the stream saw these corks thus floating past, he could infer that they had been thrown in at regular intervals; and, moreover, if he knew the rate of the stream, and that the corks were thrown in by a person standing at the river's edge, he would know that the interval between the throwing of successive corks was one second. But, *vice versâ*, if he knew the rate of the stream, and that the corks were thrown in at intervals of one second, he could infer that the person throwing them was standing still. For let us consider what would happen, if the cork-thrower sauntered up-stream or down-stream while throwing corks at intervals of one second. Suppose he moved up-stream at the rate of a foot per second; then, when he has thrown one cork, he moves a foot up-stream before he throws the next; and the first cork has floated three feet down stream; hence the second cork falls four feet behind the first. Thus the common distance between the corks is now four feet instead of three feet. Next suppose he saunters down-stream at the rate of a foot per second; then, when he has thrown one cork, he moves a foot down-stream before he throws the next; and the first cork has floated three feet down-stream; hence the second cork falls only two feet behind the first. Thus the common distance between the corks is now two feet instead of three feet. It is clear, then, that the person standing a mile or so down-stream, if he knows that the stream is flowing three feet per second, and that his friend up-stream is throwing one cork in per second, can be quite sure that his friend is standing still if the corks come past with a common interval of three feet between them. Moreover, he can be equally sure that his friend is sauntering up-stream if the corks come past with a common interval exceeding three feet; and that he is sauntering down-stream, if the common interval is less than three feet. And if, by some process of measuring, he can find out exactly *how much* greater or how much less than three feet the interval is, he can tell exactly how fast his friend is sauntering up-stream or down-stream. It would not matter how far down-stream the observer might be, so long as the stream's rate of flow remained unchanged; nor, indeed, would it matter, even though the stream flowed at a different rate past the observer than past the cork-thrower, so long as neither of these two rates were liable to alteration.

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[1] I am indebted for the illustration on which is based the explanation which follows, to my friend and college contemporary, Mr. Baily, great-nephew of the eminent astronomer, Francis Baily.

Now, we may compare the emission of light-waves by a luminous object to the throwing of corks in our illustrative case. The rate of flow for light-waves is indeed infinitely faster than that of any river, being no less than 185,000 miles per second. The successive light-waves are set in motion at infinitely shorter time-intervals, since for extreme red light there are no less than 458,000,000,000,000 undulations per second, and for extreme violet no less than 727,000,000,000,000; but these specific differences do not affect the exactness of the illustration. It is obvious that all that is necessary to make the parallel complete is that the flow of light-waves shall reach the observer at a constant rate (which is the actual case), and that he shall know, in the case of any particular and distinguishable kind of light, what is the rate at which the wave-action is successively excited, and be able to compare with this known rate the rate at which they successively reach him. If they come in quicker succession than from a luminous body at rest, he will know that the source of light is approaching as certainly as our observer down-stream would know that his friend was sauntering towards him if the corks came two feet apart instead of three feet. If, on the contrary, the light-waves of a particular kind come in slower succession than from a body at rest, the observer will know that the source of light is receding, precisely as the river-side observer would know that his friend was travelling away from him if the corks came past him four feet apart instead of three.

Now, the stellar spectroscopist *can* distinguish among the light waves of varied length which reach him, those which have a particular normal length. He analyses star-light with his spectroscope, and gets from it a rainbow-tinted streak crossed by dark lines. These dark lines belong to definite parts of the spectrum; that is, to such and such parts of its red, or orange, or yellow, or green, or blue, or indigo, or violet portion. Thus they correspond to light having a particular wave-length. And *many* of these lines in stellar spectra are identifiable with the lines due to known elements. For instance, in the spectrum of Sirius there are four strong dark lines corresponding to the known bright lines of the spectrum of hydrogen. Thus the wave-length corresponding to any one of these dark lines is perfectly well known to the spectroscopist from what he has already learned by examining the bright lines of hydrogen. Now, if Sirius were receding very rapidly, the wave-length corresponding to one of these lines would be lengthened; it would correspond, in fact, to a part of the spectrum nearer the red end or the region of longer light-waves, and thus the dark line would be shifted towards the red end of the spectrum: whereas, on the contrary, if Sirius were very rapidly approaching, the dark line would be shifted towards the violet end of the spectrum. All that would be necessary would be that the rate of approach or recession should bear an appreciable proportion to the rate at which light travels, or 185,000 miles per second. For, reverting to our cork-thrower, it is clear that if he travelled up-stream or down-stream at a rate exceedingly minute compared with the stream's rate of flow, it would be impossible for the observer down stream to be aware of the cork-thrower's motion in either direction, unless, indeed, he had some very exact means of measuring the interval between the successive corks.

Now the spectrum of a star can be made longer or shorter according to the dispersive power employed. The longer it is, the fainter its light will be; but, so long as the dark lines can be seen, the longer the spectrum is, the greater is the shift due to stellar recession or approach; and therefore the more readily may such recession or approach be detected. But, with the instrument used by Dr. Huggins four years ago, it was hopeless, save in the case of the brilliant Sirius (giving more than five times as much light as any other star visible in our northern heavens), to look for any displacement due to a lower rate of recession than some hundred miles per second (little more than the two-thousandth part of the velocity of light). What was to be done, then, was to provide a much more powerful telescope, so that the stellar-spectra would bear a considerably greater degree of dispersion. With admirable promptitude the Royal Society devoted a large sum of money to the construction of such an instrument, to be lent to Dr. Huggins for the prosecution of his researches into stellar motions of approach and recession. This telescope, with an aperture of fifteen inches, and a light-gathering power somewhat exceeding that usual with such an aperture, was accordingly completed, and provided with the necessary spectroscopic appliances. Many months have not passed since all the arrangements were complete.

In the meantime, I had arrived at certain inferences respecting the proper motions of the stars, on which Dr. Huggins's researches by the new method seemed likely to throw an important light.

More than three years ago, I had expressed my conviction that whenever the recorded proper motions of the stars were subjected to a careful examination, they would confirm the theory I had enunciated, that the stars are arranged in definite aggregations of various forms—star-groups, star-streams, star-reticulations, star-nodules, and so on.[1] Making leisure, in the summer of 1869, for entering upon such an examination I was led to several results, which not only confirmed the above-mentioned theory but suggested relations which I had not hitherto thought of. Some of these results are discussed in the essay called 'Are there any Fixed Stars?' which precedes the present paper; others are presented in the essay called 'Star-Drift.' The special results on which Dr. Huggins's recent discoveries throw light, were first publicly announced in a paper read by me before the Royal Society, on January 20, 1870.

I had constructed a chart in which the proper motions of about 1,200 stars were *pictured.* To each star a minute arrow was affixed, the length of the arrow indicating the rate at which the star is moving on the celestial vault, while the direction in which the arrow pointed shows the direction of the star's apparent motion. This being done, it was possible to study the proper motions much more agreeably and satisfactorily than when they were simply presented in catalogues. And certain features, hitherto unrecognised, at once became apparent. Amongst these was the peculiarity which I have denominated 'star-drift'; the fact, namely, that certain groups of stars are travelling in a common direction.[1] This was indicated, in certain cases, in too significant a manner to be regarded as due merely to chance distribution in these stellar motions; and I was able to select certain instances in which I asserted that the drift was unmistakable and real.

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[1] I remind the reader that the essay 'Notes on Star-Streams' appeared in the 'Intellectual Observer' for August 1867, 'Notes on Nebulæ' in 'The Student' for March 1868, and 'A New Theory of the Universe' in 'The Student' for February, March, and April 1869.

Amongst these instances was one of a very remarkable kind. The 'seven stars' of Ursa Major—the Septentriones of the Ancients—are known to all. For convenience of reference, let us suppose these seven divided as when the group is compared to a waggon and horses. Thus, there are four waggon-wheels and three horses. Now, if we take the waggon-wheels in sequence round their quadrilateral (beginning with one of the pair farthest from the horses), so as to finish with the one which lies nearest to the horses—these are named by astronomers in that order Alpha, Beta, Gamma, and Delta of the Great Bear. Thus, Alpha and Beta are the well-known pointers (Alpha nearest the pole), and Delta is the faintest star of the Septentrion set. The three horses are called in order Epsilon, Zeta, and Eta; Epsilon being nearest to Delta. Now when the proper motions of these seven stars had been mapped, I found that whereas Alpha and Eta are moving much as they would if the Sun's motion were alone in question, the other five are all moving at one and the same rate (on the star-sphere, that is) in almost the exactly opposite direction. Moreover, a small star close by Zeta (the middle horse), a star known to the Arabian astronomers as the 'Test,' because to see this star was held a proof of good eyesight, is moving in the same direction and at the same rate as Zeta and the rest of this set. And besides this star (which has also been called Jack by the Middle Horse), Zeta has a telescopic companion which accompanies him in his motion on the celestial sphere.

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[1] I include this among 'features hitherto unrecognised,' though Michell had already noted the fact that the stars are arranged into systems. 'We may conclude,' he said, 'that the stars are really collected together in clusters in some places, where they form a kind of systems; whilst in others there are few or none of them, to whatever cause this may be owing, whether to their mutual gravitation or to some other law or appointment of the Creator.'

After a careful consideration of these circumstances, and an analysis of the probabilities in favour of and against the theory that the concurrence of apparent motion was merely accidental, I came to the conclusion that the five large stars and the two smaller ones form a true drifting set. I found that, on a moderate computation, the odds were upwards of half a million to one against the concurrence being accidental; and since I had recognised other instances of concurrence not less striking, I felt that it was morally certain that these stars belong to one star-family.

The reader will perhaps not be surprised to learn, however, that before publishing this conclusion I submitted it (in July 1869) to one who was, of all men, the best able to pronounce upon its significance—the late Sir John Herschel. I have the letter which he sent in reply (dated August 1, 1869), before me as I write. The part relating to my discovery runs as follows :—'The considerations you adduce relative to the proper motions of the stars are exceedingly curious and interesting. Of late years catalogues have gone into much detail, and with such accuracy that these motions are of course much better known to us than some twenty or thirty years ago. The community of proper motions over large regions (of which you give a picture in Gemini and Cancer) is most remarkable, and the coincidence of proper motion in Beta, Gamma, Delta, Epsilon, and Zeta Ursæ Majoris most striking. Your promised paper on this subject cannot fail to be highly interesting.'[1]

In a letter written on May 11, 1870, and referring not to another letter of mine, but to my 'Other Worlds,' Sir John Herschel remarked, 'the cases of star-drift such as that in Ursa Major are very striking, and richly merit further careful examination.'

My first public expression of opinion respecting the star-drift in Ursa Major was conveyed in the following terms:—'If these five stars indeed form a system (and I can see no other reasonable explanation of so singular a community of motion), the mind is lost in contemplating the immensity of the periods which the revolutions of the components of the system must occupy. Mädler had already assigned to the revolution of Alcor around Mizar (Zeta Ursæ) a period of more than 7,000 years. But if these stars, which appear so close to the naked eye, have a period of such length, what must be the cyclic periods of stars which cover a range of several degrees upon the heavens.' (From Zeta to Beta is a distance on the heavens of about nineteen degrees.) 'The peculiarities of the apparent proper motions of the stars,' I added, 'lend a new interest to the researches which

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[1] He proceeds as follows (the passage is removed from the main text as relating to a different branch of the subject):—'I cannot say that I am at all surprised at its being found that the average proper motions of stars of small magnitudes is not less than those of large, considering (as I have always done) that the range of individual magnitude (i.e. lustre) must be so enormous that multitudes of *very* minute stars may in fact be our very near neighbours.' Compare my paper on 'The Sun's Journey through Space,' above referred to, which paper also deals with the point touched on in the next sentence of Sir John Herschel's letter:—'Your remark on the conclusion I have been led to draw relative to the small effect of the correction due to the sun's proper motion, will require to be very carefully considered, and I shall of course give it every attention.'

Dr. Huggins is preparing to make into the stellar proper motions of recess and approach.'[1]

But a few months later, in a lecture delivered at the Royal Institution, I pointed out more definitely what result I expected from Dr. Huggins's researches. 'Before long,' I said, 'it is likely that the theory of star-drift will be subjected to a crucial test, since spectroscopic analysis affords the means of determining the stellar motions of recess and approach. The task is a very difficult one, but astronomers have full confidence that in the able hands of Dr. Huggins it will be successfully accomplished. I await the result with full confidence that it will confirm my views.'[2]

It will be manifest that if the five large stars in Ursa are really travelling in the same direction, then, when Dr. Huggins applied the new method of research, he would find that, so far as motion in the line of sight was concerned, these stars were either all receding or all approaching at the same rate, or else that they were all alike in showing no signs of any motion, either of recess or approach.

But in the meantime there was another kind of evidence which the spectroscope might give, and on which I formed some expectations. If these stars form a single system it seemed likely that they would all be found to be constituted alike—in other words, that their spectra would be similar. Not indeed that associated stars always display such similarity. Indeed the primary star of a binary system not unfrequently exhibits a spectrum unlike that of the small companion. But the five large stars in Ursa, being obviously primary members of the scheme they form, might be expected to resemble each other in general constitution. Moreover, since the stars not included in the set—viz.,

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[1] 'Proceedings of the Royal Society,' January 20, 1870, pp. 170, 171.

[2] 'Report of the Royal Institution of Great Britain,' Weekly Evening Meeting, Friday, May 6, 1870, p. 7.

Alpha and Eta—might be regarded as probably very much nearer or very much farther away, it was to be expected (though not so confidently) that these two stars would have spectra unlike the spectrum common (on the supposition) to the five stars.

Now, Secchi announced that the stars of the Great Bear, with the exception of Alpha, have spectra belonging to the same type as the spectrum of the bright stars Sirius, Vega, Altair, Regulus, and Rigel. This result was in very pleasing accordance with the anticipations I had formed, except that I should rather have expected to find that the star Eta had a spectrum unlike that of the remaining five stars of the Septentriones. Moreover, as the stars belonging to this particular type are certainly in many cases, and probably in all, very large orbs[1] (referring here to real magnitude, not to apparent brilliancy), the inference seemed fairly deducible that the drifting five stars are not nearer than Alpha, and therefore (since we have seen that it is unlikely that *all* the Septentriones lie at nearly the same distance) the inference would be that the drifting stars lie much farther away than the rest.

It remained, however, that the crucial test of motion-measurement should be applied.

In the middle of May last I received a letter from Dr. Huggins announcing that *the five* are *all receding* from the earth. In all the hydrogen line called F, is 'strong and broad.' In the spectrum of Alpha the line F is 'not very strong' (so faint, indeed, Dr. Huggins afterwards informed me, that he preferred to determine the star's motion by one of the lines due to magnesium in the star's atmosphere. He found that Alpha is *approaching*. As to Eta, Dr. Huggins remarked that the line at F is 'not so strong or so broad' as in the spectrum of 'the five.' He was uncertain as to the direction of motion, and mentioned that 'the star was to be observed again.' He subsequently found that this star is receding. But whereas all the five are receding at the enormous rate of 20 miles per second, Eta's recession was so much smaller that, as we have seen, Dr. Huggins was unable to satisfy himself at a single observation that the star was receding at all.

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[1] Sirius demonstrably gives out much more light than our Sun, and according to the best determinations of his distance he must (if his surface is of equal intrinsic lustre) be from 2,000 to 8,000 times larger than the Sun. Vega, Altair, and Rigel are also certainly larger and may be very much larger than our Sun.

It will be seen that my anticipations were more than fulfilled. The community of recessional motion was accompanied by evidence which might very well have been wanting—viz., by the discovery that neither Eta nor Alpha shared in the motion. Moreover, the physical association between the five stars was yet further evidenced by the close resemblance found to exist between the spectra of the five stars. Dr. Huggins remarked in his letter, 'My expectation had nothing to do with the above results. At the moment, I thought Alpha was included in the group, and was therefore a little disappointed when I found Beta going the opposite way.'

We have at length, then, evidence, which admits of no question—so obviously conclusive is it—to show not only that star-drift is a reality but that subordinate systems exist within the sidereal system. We moreover recognise an unquestionable instance of a characteristic peculiarity of structure in a certain part of the heavens. For, though star-drift exists elsewhere, yet every instance of star-drift is quite distinct in character—the drift in Cancer unlike that in Ursa, and both these drifts unlike the drifts in Taurus, and equally unlike the drift in Aries or Leo. Much more, indeed, is contained in the fact now placed beyond question, than appears on the surface. Rightly understood, it exhibits the sidereal system itself as a scheme utterly unlike what has hitherto been imagined. The vastness of extent, the variety of structure, the complexity of detail, and the amazing vitality, on which I have long insisted, are all implied in that single and, as it were, local feature which I had set as a crucial test of my theories. I cannot but feel a strong hope, then, that those researches which my theories suggest, and which I have advocated during the last few years, will now be undertaken by willing observers. The system of star-gauging, which the Herschels did little more than illustrate (as Sir W. Herschel himself admitted), should be applied with telescopes of different power to the whole heavens,[1] not to a few telescopic fields. Processes of charting, and especially of equal-surface charting, should be multiplied. Fresh determinations of proper motion should be systematically undertaken. All the evidence, in fine, which we have, should be carefully examined, and no efforts should be spared by which new evidence may be acquired. Only when this has been done will the true nature of the galaxy be adequately recognised, its true vastness gauged, its variety and complexity understood, its vitality rendered manifest. To obtain, indeed, an absolutely just estimate of these matters, may not be in man's power to compass; but he may hope to obtain a true relative interpretation of the mysteries of the stellar system. If any astronomer be disposed to question the utility or value of such researches, let him remember that Sir W. Herschel, the greatest of all astronomers, set 'a knowledge of the constitution of the heavens,' as 'the ultimate object of his observations.'

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[1] This is a work in which telescopes of every order of power would be useful. The observations, also, would be very easily made and would *tell* amazingly.

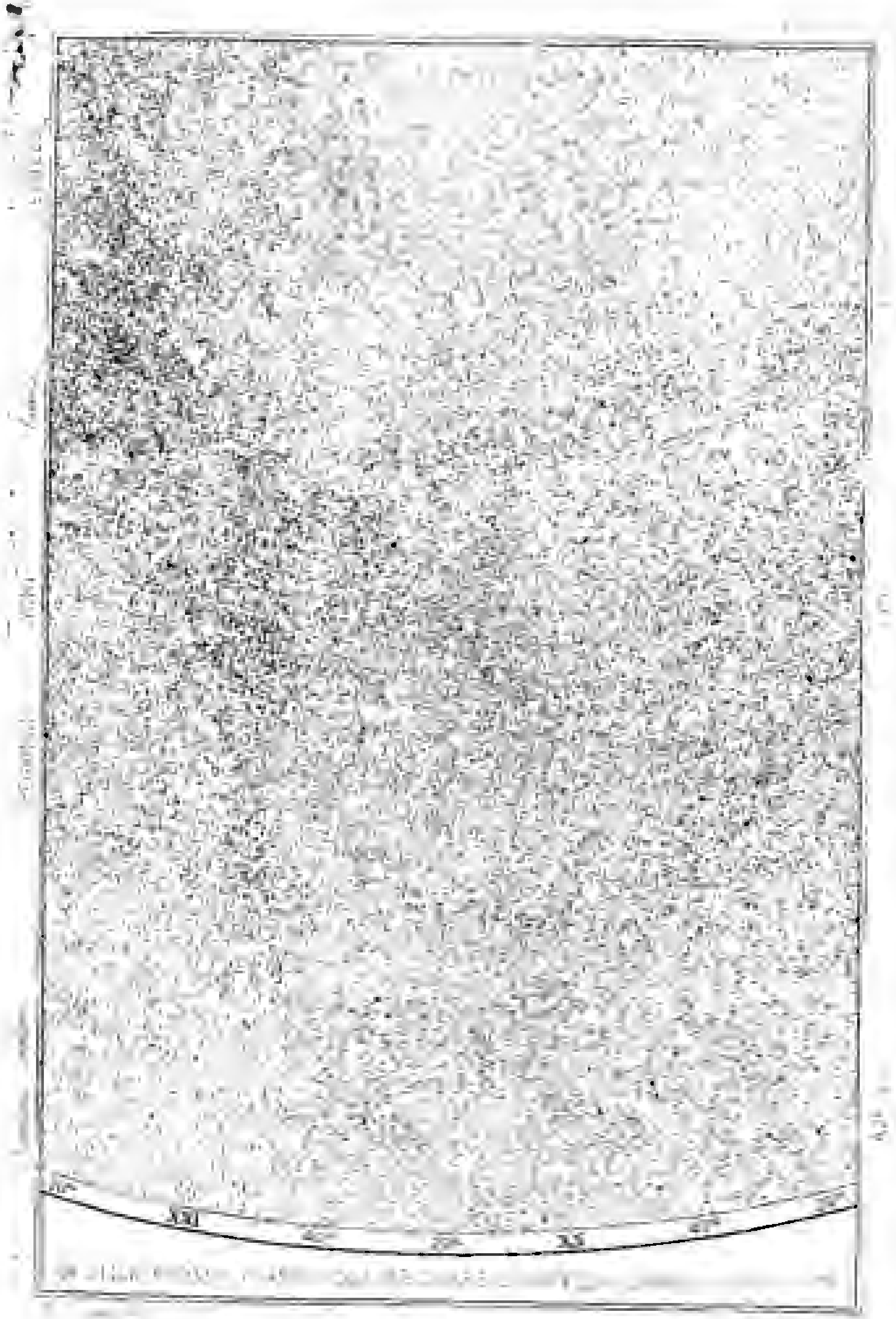
From the *Popular Science Review* for October 1872.

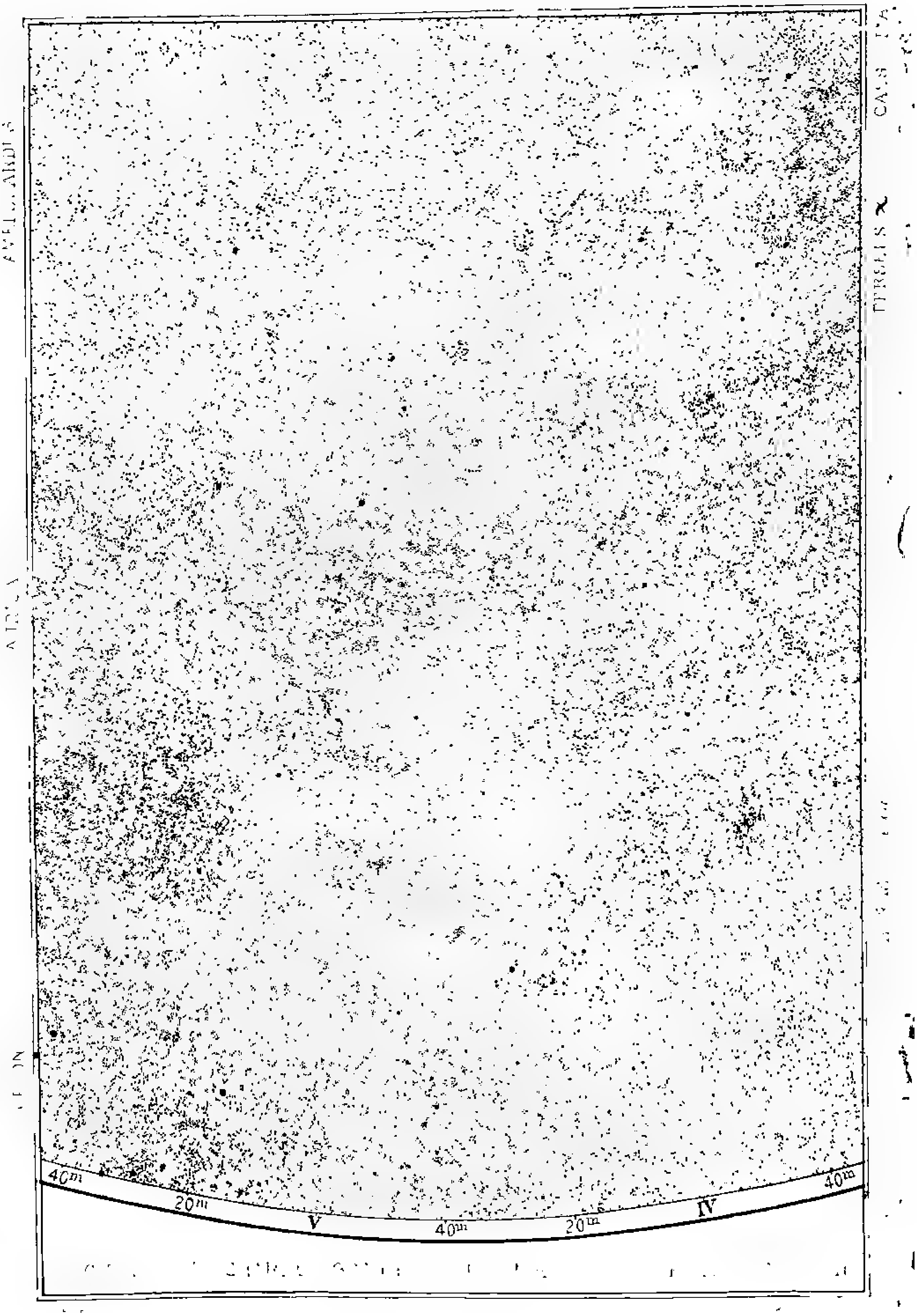
ON A CHART OF 324,198 *STARS.*

It is well known that, according to the theory of the universe enunciated by Sir W. Herschel in 1785,—that theory which is commonly, but erroneously, described as the outcome of his labours amid the star-depths,—the stars visible to the unaided eye are included within a space which bears an exceedingly minute proportion to the dimensions of the galaxy itself. Herschel's original drawing of a section of the galaxy—as conceived by him in 1785—is before me as I write. It extends across a folding sheet in a quarto volume. On the same scale, he says, the distance of the nearest fixed star would be no more than the 80th part of an inch, 'so that probably all the stars which in the finest nights we are able to distinguish with the naked eye may be comprehended within a sphere,' in the middle of this picture of our galaxy, '*of less than half a quarter of an inch radius.*'

It was with reference to this view that in the paper called 'Notes on Star Streams,' which forms the first of the present series, I pointed out that the stars of the first five orders of magnitude, only, show too marked a tendency to follow the direction of that great star-stream, the Milky Way, for the flat-disc theory to be admissible. I then noted,—and I am careful to mention the point, because objections have been urged which would imply that I had forgotten it,—that the first five orders of stars could give but very imperfect evidence *if* the accepted views about star distribution were just. But 'it is on that very fact,' I added, 'that I wish to dwell. If any connexion *does* appear between the configuration of our galaxy, and the arrangement of stars which are assumed to be much nearer to us than the Milky Way, it will be obvious that we must somewhat modify our views.'

I was not then aware that the astronomer Piazzi had noted the same fact,—though he had dealt rather with statistical evidence than with the more direct evidence I adduced from my star-charting. Nor was I then aware that Struve had followed up Piazzi's hint in the introduction to the 'Catalogus Regiomontanus,' and that he had been led to precisely my own conclusion,—viz., that the observed distribution of the stars of the leading orders of magnitude is incompatible with the accepted or text-book theory of the Sidereal System. Still less was I then aware, though I had carefully read through all Sir W. Herschel's papers on the heavens, that in his later papers he virtually abandoned the theory of 1785. This fact had wholly escaped my notice, as I believe it escapes the notice of nearly all who give to those papers but a single perusal, however carefully that perusal may be made. It was only on a careful re-investigation of the whole series of Sir W. Herschel's papers (well worth and indeed requiring many readings) that I observed how completely his views changed some fifteen years or so after the enunciation of the theory of 1785. Somewhat later I found that Struve had been led, by a second reading of Herschel's papers, to a similar conclusion. For, in his 'Études d'Astronomie Stellaire,' in which he extended his statistical inquiries to stars down to the 9th magnitude, he remarks that he 'had been chiefly led to this ulterior discussion by a re-examination (*une*

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nouvelle étude) of the memoirs of Sir W. Herschel,' which 'has led me to the conviction,' he adds, that my actual ideas respecting the Milky Way are only opposed to Herschel's theory of 1785, but agree very well with the later views (*sont extrêmement conformes aux vues postérieures*) of that great astronomer.'

The researches I am now dealing with may be regarded as an extension of Struve's inquiry; only that whereas he employed a method of statistical research, I have preferred the method of actual charting to which I have adhered throughout these inquiries—whether I have dealt with stars or nebulæ. I believe that star-charting, appealing as it does directly to the eye, and presenting not only general features, but those minor details which the statistician is obliged to neglect, is well worth the increased labour which it involves.

In order that the bearing of the present inquiry on Struve's, as well as on the Herschelian star-gauges, may be rightly apprehended, it will be as well to note what Struve and the Herschels actually accomplished, the more so that mistaken notions are very prevalent on the subject.

For instance, with respect to the star-gaugings, the opinion is not uncommonly entertained that the whole sphere of the heavens was surveyed or gauged by the Herschels. Many persons seem unaware of the fact that such a survey would have been quite impracticable even though these great astronomers had been able and willing to give their whole time to the work. When we remember that the labour actually bestowed by the Herschels on their star-gaugings bore but a minute proportion to that which they bestowed on other and more difficult researches, we need not be surprised to find that the portion of the heavens actually gauged was exceedingly small compared with the whole surface of the celestial sphere. To afford an idea of the real extent of the Herschelian gauges, the following illustration may be employed :—If the whole surface of the celestial sphere be represented by the surface of an ordinary chess-board, then all the gauge-fields of Sir W. Herschel taken together (though 3,400 in number) correspond to but about the fourth part of one of the squares: while the gauge-fields surveyed by Sir John Herschel, taken together, correspond to somewhat less than a fifth part of one of the squares. Again, all the stars actually counted by Sir W. Herschel during his star-gauging amounted to about 90,000; those counted by Sir John Herschel to about 70,000: in all about 160,000. Now Struve estimates the total number of stars visible with the Herschelian gauging telescopes at upwards of 20 millions; so that the number actually counted is less than the 120th part of those which would have come into view in a complete survey. To this I may add that Sir W. Herschel, speaking of his 3,400 gauges, says that he regards them as 'only an example to illustrate the spirit of the method.'

Struve's latest and most extensive series of inquiries was applied to a list of 31,085 stars occupying a zone of the heavens between 15° north and 15° south of the equator, covering, therefore, a space equal to about the fourth part of the celestial sphere. By a series of ingenious inquiries into the number of stars probably missed in various parts of this zone, Struve virtually increased the number of stars he dealt with from about 31,000 to about 52,000.

The method he then adopted in dealing with these stars was simply this: he divided the great equatorial zone over which they were spread into hours of right ascension, and compared the numbers of stars in these divisions. He regarded these numbers as fairly indicating the richness of star-distribution *round the equator itself, compressing (as it were) a zone 30° wide into an indefinitely thin equatorial ring*. Finally, *he converted this equatorial ring into an equatorial disc, by* *conceiving the stars of the various orders of magnitude to be distributed at corresponding distances from the centre of the celestial sphere.*

With all respect for the eminent abilities of the great Russian astronomer, I venture to point out that there are very grave objections to his method of research. The spaces into which he divided the heavens were inordinately large: each, in fact, extending 15° in right ascension and 30° in polar distance. To take averages for divisions so extensive must surely be regarded as unsatisfactory.

Moreover, the galaxy, regarded by Struve as a zone of stellar concentration, runs obliquely across his wide equatorial zone, a circumstance obviously tending to render his results less instructive than they might otherwise have been. He notes this himself in accounting for the absence in his statistical results of all signs of the division of the galaxy.

Lastly, as the main result of Struve's inquiries was to exhibit the want of uniformity in stellar distribution *over* the equatorial zone, his assumption of a uniform distribution *in the plane of* the equatorial disc, must be regarded as venturesome in the extreme.

As respects the construction of the chart, specimens from which form Plates XI. and XII., I need here say little, because I have elsewhere adverted to the points which seem of chief importance. I would note, however, that the pencilled divisions on this projection represent spaces on the sphere averaging in extent one 280th part, only, of those into which Struve divided his equatorial zone. Each space, also, was filled in, *not* according to the mere number of stars, but very carefullly by an eye-draught from the corresponding space in Argelander's charts. That I might not be influenced by the natural tendency (as Sir John Herschel calls it) to 'exaggerate any peculiarity which might at the moment seem a feature,' each meridional sector, 5° wide, was covered over as soon as it was completed. The work when once commenced was carried on without intermission (except for rest and food), all other work being set aside. The necessity for this will be obvious when it is remembered that if the work had been left even for a few days only, the probability is that, on returning to it, I might have somewhat modified the scale of star-magnitudes, a circumstance which would have rendered all the work valueless. Even as it was, I found continual watchfulness necessary lest any change in the style of the work should be unwittingly admitted. It is not easy to work at a star-map for six or seven weeks in succession without changing the style or character of the charting; and I found it quite impossible to complete the chart in less than six or seven weeks. In fact, it is only necessary to compute the time required for marking in the stars to see that the work could not readily be compressed into a shorter interval of time. At the moderate rate of one minute for ten stars, 32,400 minutes, or 540 hours would be required; but the time actually occupied amounted only to 400 hours. I was pleased to find that along the line in which the last day's work was brought up to the first day's, no signs of any change in the style of mapping could be noticed. Had the star-magnitudes been enlarged or diminished in the interval, a sharp line of demarcation would unquestionably have been recognisable. As it is, I think I may defy the acutest observer to determine along what radial line the work began and ended.

So much being noted, in order that the trustworthiness of the chart may be sufficiently established, I will now briefly sum up the results to which the careful study of the chart appears to lead.

In the first place, Struve's general conclusion that the stars of the first nine or ten orders of magnitude are more densely aggregated along the galactic zone is abundantly justified.[1] But instead of a gradual increase of density such as his statistics suggested, we recognise in the chart a distinctly marked aggregation within those very regions of the heavens where the Milky Way is brightest to the eye. In other words, we have clear evidence that it is not towards a certain *zone* that the stars are gathered, but into those irregular cloudlike masses, those streams, projections, and interlacing branches which constitute the Milky Way, as it is actually presented on clear nights to our study.

Let me quote here—in correction of that account of the Milky Way which is too often presented in our text-books—the description which Prof. Nichol has given of that wonderful star region. 'It is, indeed, only to the most careless glance,' he justly says, 'or when viewed through an atmosphere of imperfect transparency, that the Milky Way seems a continuous zone. Let the naked eye rest thoughtfully on any part of it, and if circumstances be favourable, it will stand out rather as an accumulation of patches and streams of light of every conceivable variety of form and brightness; now side by side; now heaped on each other; again spanning across dark spaces, intertwining, and forming a most curious and complex net-work; and at other times darting off into the neighbouring skies in branches of capricious length and shape, which gradually thin away and disappear.'

It will be seen that the aspect of the star-groups in the chart accords in the most significant manner with this description.

I would note, next, a circumstance which at first somewhat surprised me. The densely crowded regions in the chart show certain very well-marked projections in Perseus and Auriga, on the northern side of the galaxy. Sir John Herschel, in his account of the Milky Way, mentions no such projections. But something in the aspect of these projecting masses seemed familiar to me so soon as I studied the complete chart. At length I was led to examine the Milky Way as depicted in the maps constructed by Sir John Lubbock. In my Gnomonic charts I had followed that figure of the galaxy, and I had used the same delineation in a work now out of print, called the 'Constellation Seasons.' On turning to my own drawings in these works, I found the very projections whose appearance had seemed familiar to me, as well as a long projection on the southern side of the Milky Way in Perseus, extending from Algol towards Andromeda. This projection also follows the direction of a declination-parallel, and its form is very clearly to be recognised in the chart. In the key-map I have shown these projections on opposite sides of the Milky Way in Perseus.

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[1] It is to be noticed that Argelander's magnitudes differ from those employed by Sir John Herschel. According to the nomenclature of the English astronomer my chart shows all stars down to the eleventh magnitude inclusive.

In the chart, however, we see these projections carried much further away from the main branch. We see, also, that from the other half of the Milky Way between Cepheus and Aquila there are similar extensions curving round on the northern side to a considerable distance from the main branch. One of these traverses Lyra and curves round almost to Ursa Major; another passes round from Hercules to Corona; while we note that the discontinuous branch which the naked eye recognises as extending from Cygnus to Ophiuchus is carried round towards Boötes.

Amongst these branching extensions there is, in places, so singular a tendency to agreement with declination-parallels, that the question may well suggest itself whether the peculiarity is not due to some feature in the construction of the maps—some bias, as it were, by which I might have been led to parallelise the star-grouping in this particular way; for plainly the north pole of the sky has no relation whatever to the construction of the stellar heavens. I am able to assert, however, with the utmost certainty of conviction, that nothing in the construction of the maps can explain the peculiarity. The original chart was most carefully divided into 26,400 spaces, each very small therefore, and the average number of stars to each space was but eighteen. These spaces were severally much smaller, both in length and breadth, than the cross-section of these branching extensions; so that even the most unpractised draughtsman, working in the most careless manner, could not have been so biassed that these curved branches could be thus explained away. As I have had some degree of practice in star-charting, and as, further, every portion of this chart was drawn as though it were forthwith to be subjected to the severest and most critical scrutiny, it will be understood how utterly impossible it is that so marked a peculiarity can have the imagined explanation.[1]

Apart from this, the curved branches will be found, on a close scrutiny, not to agree exactly with the direction of declination-parallels; and all of them can be traced in Argelander's large charts, when these are carefully studied.

A circumstance of some interest is to be recognised in the fact that these branching extensions are found to lead, in almost every instance, towards regions of the heavens where many nebulæ exist.

The rich stellar region of Orion, as well as the Hyades and Pleiades, are shown to be intimately associated with the Milky Way.

In conclusion, I may remark that although the results to which this chart seems to point may appear altogether opposed to accepted views respecting the construction of the heavens, they accord exceedingly well with the enlarged views formed by Sir W. Herschel towards the close of his career as an observer,[1] and even better with the views towards which his later researches *pointed.* The mode of research I have adopted is altogether new, and it is one which seems more likely to lead to clear views of the construction of the heavens than any yet employed; for it presents before the eye at one view what hitherto has had to be inferred from tabulated figures. It is capable of a far wider extension, however; and I could wish that others who have more leisure than I can afford would aid in this interesting branch of inquiry. To such I would repeat the words by which Sir John Herschel, two years ago, encouraged me to fresh inquiries: 'Be not deterred,' he wrote on August 1, 1869, 'from dwelling closely and consecutively on these inquiries, by any idea of their hopelessness which the objectors against "paper astronomy" may entertain, or by the real slenderness of the material threads out of which any connected theory of the universe has at present to be woven.'

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[1] This does not preclude the possibility that there may be in places a tendency to 'meridional' or 'parallel' *graining*, so to speak; but of a much finer nature than the peculiarity referred to in the text, and in no way affecting the chart's real significance.

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[1] I except the papers of 1817 and 1818.

From the *Monthly Notices of the Royal Astronomical Society*, November 1871.

ON TWO RICH NEBULAR REGIONS.

In the maps which illustrate my papers on the distribution of the nebulæ, in my 'Essays on Astronomy,' the nebulæ are not shown in their true places, but are simply arranged according to their numerical distribution in spaces having given limits in R.A. and Dec. While engaged in the construction of these maps, it occurred to me that considerable light might perhaps be thrown on questions relating to stars and nebulæ, if maps were constructed on an adequate scale, showing the stars down to telescopic orders of magnitude, and all the known nebulæ, properly placed. It is clear that to construct such maps for the whole heavens would be a work involving several months of labour, and it is seldom that I can find even a few hours of leisure for such work. I have, however, begun to prepare maps of the most interesting nebular regions, and I now present two such maps—one showing the rich nebular region in the wing of Virgo, the other the rich region in Coma Berenices. The former map contains upwards of 250 nebulæ, the latter containing some 80; and in both maps all the stars in Argelander's series of charts (corrected for precession, so as to accord with the date of Sir John Herschel's Catalogue of Nebulæ) have been included. Thus the maps show all the stars down to Herschel's 11th magnitude, or to the magnitude intermediate to Argelander's 9th and 10th.[1]

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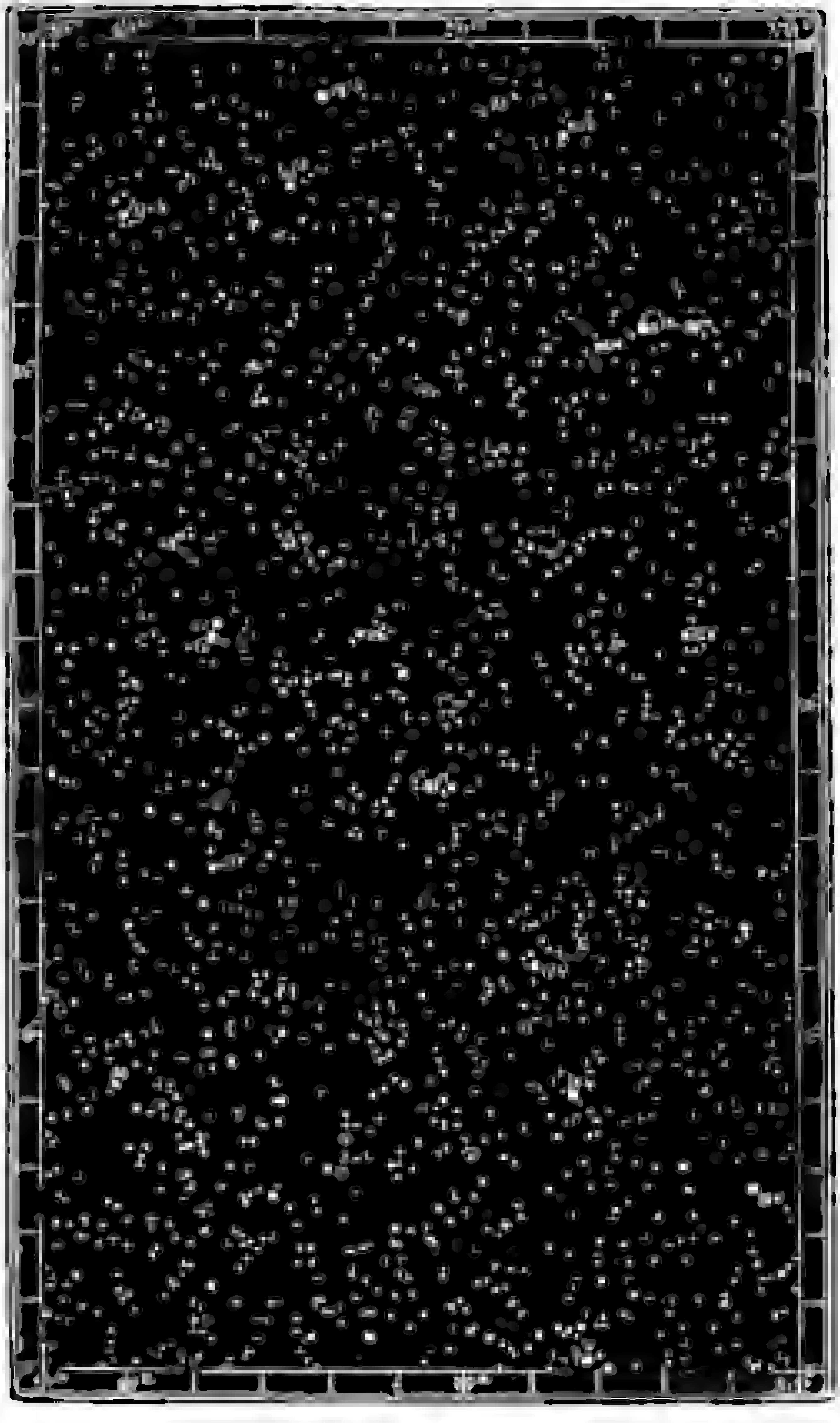
[1] I say down to these magnitudes; but there are many who prefer to speak of the fainter orders of stars as belonging to the higher orders of magnitude. It



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The larger map (fig. 14), extends from $12^h\ 0^m$ to $12^h\ 45^m$ in R.A., and from the equator to 20° in north declination. Thus it covers about $\frac{1}{195}$th part of the whole heavens, and the average proportion of nebulæ for that extent of surface would be about 28. Since it contains more than 250, its relative richness is unmistakable.

The smaller region (see fig. 15), extends from $12^h\ 32^m$ to $13^h\ 12^m$ in R.A., and from 23° to 38° in north declination. In the most northerly part of the map, however, the range in R.A. is about a degree and a half greater. The area is about $\frac{1}{360}$th part of the heavens. The average proportion of nebulæ would be 15, the actual number is more than five times as great.

In drawing these maps, I have been much struck by a circumstance which may perhaps not seem particularly well marked on a casual examination of the maps, but which has a real existence nevertheless. The stars are not arranged uniformly over either region, but to some degree clusteringly with interspersed spaces relatively vacant. Now no nebulæ appear in the more vacant spaces, nor do nebulæ appear chiefly where the stars are most clustered. It is on the borders of star-clusterings, and in the breaks of star-streams, that the nebulæ show themselves, precisely as though they had taken the place of stars where star-matter began to fail.

This result is in agreement with what Sir W. Herschel observed as to the relation between nebulæ and the fainter telescopic stars; for it was not in absolutely starless regions

would be well that some definite rule should be adopted in this matter. My own sole reason for speaking of the brighter orders as the higher orders is, that I find that mode of speaking to be more in accordance with ordinary ideas on the subject than that which calls a very faint star a star of a high order of magnitude. Authority is divided on the subject, some astronomers adopting one mode of speaking, while others use the other. Hence some confusion is apt to result.

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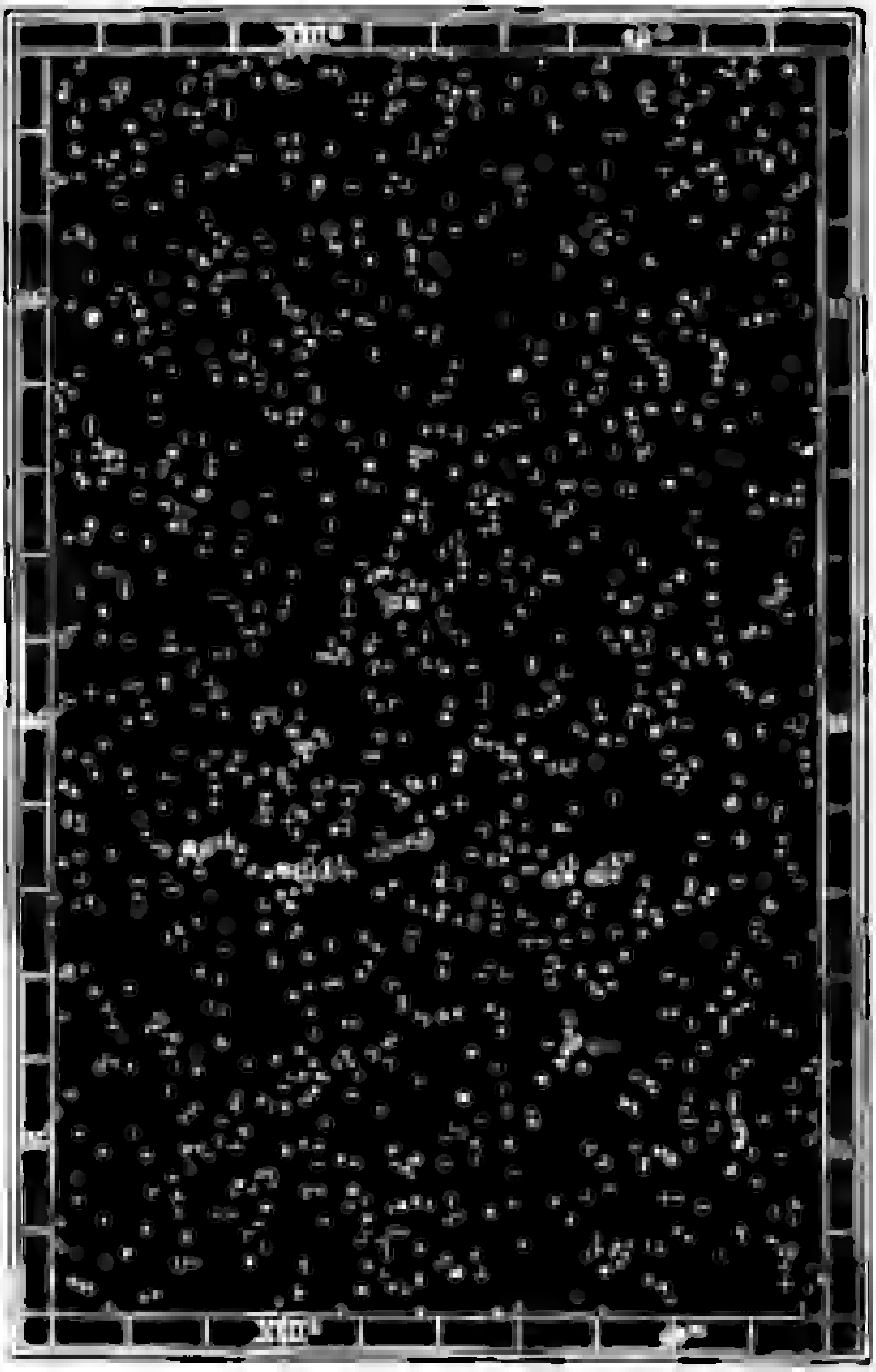

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I cannot but regard the relation here stated to exist in the maps as full of significance. After going carefully over the whole ground in mapping, I have become convinced that the peculiarity has a real existence, that it cannot be ascribed to chance. The peculiarity recognised by Sir W. Herschel is equally striking, and we know that he was by no means disposed to regard it as merely accidental. Taking the two features, or rather the two forms of one and the same feature, into due account, it seems to me impossible to doubt that there is in these regions a real association between nebulæ and stars. The nebulæ here, at any rate, would seem not to be external star-systems.

If it should at any time become possible to compare the proper motions of these nebulæ (some of which are gaseous) with those of the stars—either the apparent proper motions in the heavens, or motions of recess or approach—evidence of importance might be obtained respecting the relative distances of stars and nebulæ in these regions. I entertain very little question as to the result of such researches. I would not venture, indeed, to assert that any marked community of motion will be recognised when nebulæ and stars are compared; but I feel confident that the average rate of apparent nebular motion will be found to correspond with the average rate of stellar motion, and that no discrepancy will be found to exist between the *general* character of the stellar and nebular motions in the direction of the line of sight.

From the *Monthly Notices of the Astronomical Society* for November 1872.

THE CONSTRUCTION OF THE HEAVENS.

A knowledge of the construction of the heavens has always been the ultimate object of my observations.—Sir W. HERSCHEL (*Phil. Trans.* for 1811).

IT may appear strange, but is nevertheless strictly true, that though the name astronomy implies the science which deals with the laws of stellar distribution, yet astronomers as a body have given less attention to the discussion of these laws, or, in other words, to the solution of the problem of the construction of the heavens, than to any other department of their science. The observation of the stars has, indeed, occupied a large share of the labours of astronomers; but such observation has been directed to the exact determination of the position of individual orbs upon the vault of heaven. At the great public observatories the prosecution of such work—the importance of which, be it understood, I am very far from questioning—has been the object of many years of patient labour by many hundreds of skilful astronomers: catalogues in which star places are recorded by hundreds of thousands are now in existence, and the magnitude, colour, proper motion, annual parallax, and other relations of multitudes of stars have been carefully indicated. But these labours have not been carried out for the purpose of obtaining information respecting the structure of the sidereal universe. They have all related to the recognition and accurate placing of individual stars; and indeed, it may be said, without exaggeration, that they have all been intended to subserve terrestrial purposes rather than to extend our knowledge of celestial relations.

And it is particularly remarkable how few of the astronomers whose names stand highest in the roll of scientific fame, have taken any considerable degree of interest in the problems presented by the stellar heavens. Hipparchus and Ptolemy studied the stars in order to obtain information respecting the earth. Copernicus neither formed nor attempted to form any theory of the sidereal system. He expressed, indeed, the opinion that the universe of stars is spherical, but he based this opinion on considerations far removed from the actual study of the stars. Kepler, in like manner, did not direct his attention to the distribution of the stars, and the evidence which that distribution may afford respecting the constitution of the heavens; he, like Copernicus, expressed an opinion respecting the sphere of stars, but the opinion was based on fanciful analogies, not on observed facts.[1] Galileo limited himself to the observation of the stars with his telescope, ennunciating only the theory that the Milky Way consists of a multitude of stars, as demonstrated by his telescopic researches. Hevelius did not discuss in any way the relations presented by the stars. Huyghens, however, in his 'Cosmotheoros,' made many judicious reflections respecting the stellar system, and advanced opinions which were carefully based on observed facts. We owe to him the definite ennunciation of the theory that the stars are suns like our own,—probably, like it, the centre of planetary schemes. Huyghens, like his predecessors, failed to discuss attentively the configuration of the star groups, whether those seen by the naked eye or those revealed in the telescope, with the purpose of thence ascertaining the laws according to which the stellar universe is constituted. Newton, Halley, Flamsteed, and their contemporaries, paid in like manner very little attention, or none at all, to that stupendous problem, the solution of which Sir W. Herschel afterwards set before him as 'the ultimate object of his observations.' And passing at a step over the interval separating Newton's day from our own times, it may be said that the Herschels and the elder Struve have been the only astronomers who have attempted to form broad and connected views respecting the structure of the universe, while only half-a-dozen names need be added, if we would include those who have given attention to the subordinate problems

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[1] His reasoning is so remarkable that I venture to quote it in this place, for it affords a very suggestive insight into the nature of Kepler's mental organisation, especially when we consider that the work in which these ideas are put forward—the 'Epitome'—was published partly in 1618 and partly in 1620, while the three laws of Kepler were published in 1609 and 1619; in other words, that he was bringing out a farrago of fanciful and baseless speculations during the very period which saw him force from Nature the key to one of the noblest of her secrets. After explaining that the stars are necessarily enclosed within the substance of a spherical shell having the sun at its centre, he proceeds to reason thus:—The radius of the concavity enclosing the sun is determined by a simple proportion. Saturn, the outermost planet, travels at a distance from the sun equal to 2,000 times the sun's radius; therefore, by the harmony of relations the distance of the sphere of the fixed stars is equal to 2,000 times the distance of Saturn from the sun. But the thickness of the crystalline (the spherical shell enclosing the stars) can also be determined. All the matter of which the universe is formed is divided into three equal parts. One-third is included in the body of the sun; another third forms the substance of the planets and of the celestial ether which fills up the space within the sphere of the fixed stars; the remaining third forms the crystalline. Now since the ether fills a space exceeding the sun's volume 64 trillions of times $[(4{,}000{,}000)^{\frac{3}{2}}]$, its density must be proportionately small compared with

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his. And the density of the crystalline must be a mean between the sun's density and that of the ether. Thus the crystalline has a density 8,000 million times $[(4,000,000)\frac{3}{2}]$ less than the sun's; and as its mass is equal to the sun's, its volume is 8,000 million times greater. Hence, since its inner diameter is known, it is easy to calculate its thickness, which is found to be equal to the 6,000th part of the sun's radius—or, according to nineteenth century measurements, to about seventy miles.

It would appear, as Struve points out, that Kepler in thus reasoning was chiefly striving to accommodate the Copernican theory with the ideas entertained in his day respecting the waters above the firmament spoken of in the Pentateuch.

associated with the great one of determining the laws of the sidereal system.

But even more remarkable than the carelessness with which nearly all the great astronomers of the last three centuries have viewed this noble problem, is the fact that attention was first fairly called to the problem, and worthy attempts were first made towards its solution, by men who were not astronomers[1] in the true sense of the term, and whose names, with a single exception, remain in undeserved obscurity. How seldom do we hear the names of Wright,

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[1] Lest this remark should be misunderstood, I may as well explain in what sense I use the word astronomer. I was somewhat roundly taken to task when in the first edition of my 'Other Worlds' I spoke of Whewell in one place, and of Humboldt in another, as not being astronomers. There cannot be any question that Whewell had a clearer insight into many astronomical subjects than many who must be described as astronomers; and a like remark applies to Humboldt, Brewster, and others, as well as to those of whom I speak above. Yet it is impossible for any astronomer to read many consecutive pages of matter written on astronomical subjects by Whewell, Humboldt, Brewster, Kant, and others, without feeling that they were emphatically *not* astronomers. What then, it may be asked, is my definition of an astronomer? It is one which immediately removes anything that might seem invidious in the distinction I draw between the above-mentioned eminent men, and others—not more eminent, some far less eminent—whom I should emphatically call astronomers. An astronomer is one who devotes the main portion of his scientific life and labour to the study of astronomy, either generally or in some special department. This definition excludes, and, as I take it, properly excludes those who turn for a while from their special branches of research to the study of some special astronomical subject, however skilfully they may treat that subject. It also excludes those (and again, as I take it, the exclusion is right and proper) who have made all science their subject. But unlike other definitions which I have heard advanced, it does not exclude any class of astronomical workers or thinkers. If we take a familiarity with any special branch of astronomical lore, whether observational or mathematical, or theoretical or historical, as essential to the character of the true astronomer, we must in every case exclude many who have given nearly the whole of their scientific life and labour to the advancement of the science of astronomy. We might thus adopt a definition excluding Adams and Leverrier, or another excluding Airy or Challis, or another excluding Huggins and De la Rue, or another excluding Dawes and Webb, and Knott and Browning. Newton could be excluded because he was not an observer of the heavens; Sir W. Herschel because he was not an adept in manipulating the formulæ of Laplace and Lagrange. In fine, we might successively exclude every student of astronomy that has ever lived, with perhaps the single exception of the younger Herschel.

Lambert, and Michell associated with those of the Herschels and Struve; how seldom, indeed, are they mentioned at all: and if the name of Kant is held in high honour, how little is this due to its association with astronomical theories! Yet I do not hesitate to say, that Wright, Kant, Lambert, and Michell did more to advance men's ideas respecting the constitution of the sidereal universe than all the astronomers who lived before the time of Sir W. Herschel.

We owe to Wright, of Durham, the enunciation of that theory of the universe which is so commonly presented in our text-books of astronomy, as representing the 'outcome,' so to speak, of the labours of the elder Herschel. Nor did Wright simply enunciate that theory; he based it on observation. All that was hypothetical in his reasoning was the idea with which he started, that the stars are arranged with a certain general uniformity throughout the sidereal system. 'It seems inconsistent,' he said, 'with the harmony observed in all the other arrangements of Nature, that one scheme of stars should be arranged with perfect symmetry, while another is scattered irregularly.' It is far safer—so Wright reasoned—to conclude that the incongruity between the aspect of the lucid stars and that of the Milky Way, is due only to the imperfect nature of our survey, both as respects extent of space and duration in time. 'When we reflect,' he proceeds, 'upon the various configurations of the planets, and the changes which they perpetually undergo, we may be assured that nothing but a like eccentric position of the stars could occasion such confusion among bodies otherwise so regular; in like manner we may conclude, that as the planetary system if viewed from the Sun would appear perfectly symmetrical, so there may be some place in the universe where the arrangement and motions of the stars may appear most beautiful. If we suppose the Sun to be plunged in a vast stratum of stars of inconsiderable thickness compared with its dimensions in other respects, it is not difficult to see that the actual appearance of the heavens may be reconciled with a harmonious arrangement of the constituent bodies of such a system with respect to some common centre, provided it be admitted at the same time that the stars have all a proper motion. In such a system it is manifest that the distribution of the stars would appear more irregular the farther the place of the spectator was removed from the centre of the stratum towards either of the sides. It is also evident that the stars would appear to be distributed in least abundance in the opposite directions of the thickness of the stratum, the visual line being shortest in those directions; and that the number of visible stars would increase as the stratum was viewed through a greater depth, until at length, from the continual crowding of the stars behind each other, it would ultimately assume the appearance of a zone of light.'[1]

It will be obvious that we have here a complete enunciation of what has been called the Grindstone Theory of the stellar system. The theory is based by Wright on observed facts, precisely as it was afterwards based by Sir W. Herschel on other observed facts. Assuredly whatever credit appertains to the invention of the theory must be in justice ascribed to Wright, who thus in 1750 reasoned out and clearly described the views to which Herschel was led in 1784. Wright, indeed, did not convince his contemporaries. Either they were unwilling to examine the reasoning he advanced, or they could not recognise its force; but neither remissness nor slowness of apprehension on their part can afford just ground for depriving Wright of the credit due to his ingenious analysis of known facts.

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[1] I have not followed here Wright's actual text, not having present access to his work. The above passage is taken from the abstract of Wright's theory in Professor Grant's excellent 'History of Physical Astronomy.'

Kant's speculations, so far as they relate to the present constitution of the universe, must be regarded as simply an extension of Wright's theories. Kant had read Wright's work, the 'Theory of the Universe,' which had been reprinted in a Hamburg journal of the year 1751, and he admits that his views respecting the universe of stars were suggested by Wright's theories; but he found himself unable to indicate 'to what extent his system is a reproduction or amplification of Wright's.' There can be no question that unconscious memory had a much larger share in the production of Kant's ideas as published in 1755, than he himself (in the absence of Wright's work to refer to) could probably have imagined.[1] The most important point to be noticed in Kant's work is his extension of Wright's speculations to other orders of systems than observation had yet revealed. Wright had considered that the nebulæ indicate the existence of other systems, not necessarily like our own star system, but of the same order in the scheme of creation. Kant considered that these star systems are members of a new system of a higher order. 'We trace here,' he added, 'the first terms of a series of worlds and systems, and these first terms of an infinite series enable us to infer the nature of the rest of the series.'[2]

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[1] Professor Huxley, in his fine essay on 'Geological Reform,' remarks that Grant, in his 'History of Physical Astronomy,' makes but the briefest reference to Kant. The reason for this may probably be found in the circumstance that, in describing Wright's ideas, Grant had already presented all those of Kant's views which could properly find a place in a history of physical astronomy. The student who is anxious to inquire into those more speculative ideas of Kant which are not to be found in Wright's 'Theory of the Universe,' should refer to Kant's original work, 'Allgemeine Naturgesichte und Theorie des Himmels; oder Versuch von der Verfassung und dem mechanischen ursprunge des ganzen Weltgebündes nach Newton'schen Grundsatzen Abgehandelt.'—Kant's Sämmtliche Werke, Bd. i. p. 207.

[2] I omit all reference to the ideas of Wright, Kant, Lambert, and others, as to the existence of central suns, simply because those ideas were purely speculative. I may add here, however, that analogy now no longer requires

Strangely enough, while Kant borrowed his views almost wholly, though unconsciously, from Wright, he seems to have been disposed to regard Lambert's views—which in reality were unlike his own—as borrowed from himself. In his correspondence with Lambert, Kant remarked in 1763, that 'the accordance between their ideas extended even to the most minute details.' It will be seen, however, that in forming this opinion Kant misinterpreted the theses of Lambert.

Lambert, like Huyghens, Wright, and Kant, regarded the stars as suns resembling our own sun in importance, and like it surrounded by planetary systems. Each sun with its family of planets formed in Lambert's theory *a system of the first order.* He considered that our sun belongs to a vast globular group or cluster of stars, forming *a system of the second order*, including all those stars, spread over all parts of the heavens, which do not belong to the Milky Way. He further held that there are many systems of the second order, and that these are not distributed throughout all space, but are all found near a certain principal plane or mean level, and being ranged one behind the other to a great depth, form by their concourse the Milky Way. This is *a system of the third order.* Analogy suggests, he added, that there are in the universe many Milky Ways; 'perhaps, for instance, the nebula in Orion may be a Milky Way, nearer to us than the rest. Should this be the case, telescopic research will reveal many others, forming together *a system of the fourth order.*' And he proceeded thence to the inference that there may be systems of yet higher orders.

It will be observed that in regarding the Milky Way as formed, not *directly* of the concourse of many stars, but of

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that we should consider every system as necessarily governed by a central orb. We now know from observation that many systems of orbs exist in space which have no denominating centre.

the concourse of many clusters of stars, Lambert formed a totally different conception of its nature than Kant had adopted. Nor was this view of Lambert's, any more than were his other opinions, merely speculative. He based it on the observed irregularity of the Milky Way, on the different richness of its various parts, and on its branching figure. In passing, it may be observed that Lambert here anticipated conclusions to which Sir William Herschel was led soon after he had abandoned the principle of star gauging.

Lambert thus describes the grandeur of the universe of systems as pictured in his theory:—'How far soever we may extend the scale,' he says, 'we must necessarily stop at last. And where? At the centre of centres, at the centre of creation, which I should be inclined to term the capital of the universe, inasmuch as thence originates motion of every kind, and there stands the great wheel in which work the teeth of all the rest. From thence the laws are issued which govern and uphold the universe; or rather, there they resolve themselves into one law, of all others the most simple. But who would be competent to measure the space and time which all the globes, all the worlds, all the worlds of worlds employ in revolving around that immense body—the Throne of Nature and the Footstool of the Divinity! What painter, what poet, what imagination is sufficiently exalted to describe the beauty, the magnificence, the grandeur, of this source of all that is beautiful, great, and magnificent; and from whence order and harmony flow in eternal streams through the whole bounds of the universe.'

Michell limited his attention to the lucid stars, not enunciating any complete theory respecting the galaxy. The most important of his theoretical views, or rather the most important of the facts which he established,[1] is that which relates to the existence of laws of arrangement (even among the stars visible to the naked eye), according to which the stars form systems, separated from each other by relatively vacant spaces. ‘We may conclude,’ he says, ‘that the stars are really collected together in clusters in some places, where they form a kind of systems; whilst in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation or to some other law or appointment of the Creator.’ He then proceeds to inquire whether the sun ‘likewise makes one of some system.’ He considers that this is probably the case, and he endeavours to separate those stars which probably belong to the same system as the sun from those which do not. ‘There are some marks,’ he says, ‘by which we may with great probability include some and exclude others—while the rest remain more doubtful. Those stars which are found in clusters and surrounded with many others at a small distance from them, belong probably

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[1] Amongst the facts which Michell established must be included the existence of binary star systems. Nothing can be more complete or demon-

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strative than Michell’s reasoning on this subject; and I can conceive no reason why the credit of the discovery should be refused to the man who deduced it by exerting the noblest of human faculties—the power of abstract reasoning, in order that it should be handed over to another (however eminent) who deduced it from direct observation. It is true Michell failed to convince his contemporaries, and that his reasoning, if it had not been subsequently confirmed by Sir W. Herschel’s observations, might have remained to this day unappreciated, save by the few. But this does not do away with the fact that Michell’s demonstration was complete; and his credit ought not to be diminished because the many could not grasp the full value of the proof. A very slight extension of such a method of judging might deprive the Herschels in their turn of the credit now (as I think unjustly) assigned to them for the discovery, to make it over to the first person who (say) by obtaining photographic records of double stars, should enable everyone to see for himself that one component circles around the other. Surely the reputation of the Herschels does not require that any credit should be assigned to them which is justly due to another. Michell has indeed been particularly unfortunate in such matters, since, to mention no other instances, the so-called Cavendish experiment for weighing the earth was not only devised by him, but the very instrument with which Cavendish conducted the experiment was constructed under Michell’s superintendence.

to other systems and not to ours. And those stars which are surrounded with nebulæ are probably only very great stars, which upon account of their superior magnitude are singly visible, whilst the others which compose the remaining parts of the same system are so small as to escape our sight. And those nebulæ in which we can discern only a few stars even with the assistance of the best telescopes, are probably systems which are still more distant than the rest.' On the other hand, he inferred that those stars 'which being placed at a greater distance from each other compose the larger constellations, and such as have few or no smaller stars near them when examined with telescopes, belong probably to our own system. Variable stars he also regarded as in all probability members of the system of stars to which our sun belongs: though the reasoning on which he based this opinion is not such as can now be admitted; for he considered their variations of brilliancy to be due to variations of distance (a cause which would necessarily be more effective in the case of stars relatively near to us); but we now know that this explanation is not the true one. He also judged red stars to be much larger and nearer than their apparent brightness would suggest, and hence inferred that they also belong to the system of stars which includes our sun.

In passing to the work of Sir W. Herschel as a student of the sidereal heavens, I cannot but express some degree of surprise that so little has been done to bring the records of his labours properly before the student of astronomy. His papers merely collected into a volume would form a most important accession to astronomical literature. But if suitably edited and illustrated by the work of his son, and of others who have succeeded in the same field of research, the volume would do more to advance the study of sidereal astronomy than any work which has been published during the last century. It must be added that nearly all that has hitherto been done in making Herschel's words and work public, has been rather an injustice to his memory than otherwise. It seems to have been supposed that his own account of his work might be treated as we should treat such a work as Sir John Herschel's 'Outlines of Astronomy,' and that extracts might be made from any part of any paper, without reference to the position which that paper chanced to occupy in the complete series. It does not seem to have been noticed that not only was there a progression in the ideas as well as in the work of the great astronomer, but that there was a complete change in his opinions during the progress of his labours. Hence views expressed by him in his earlier papers are not uncommonly in strong contrast with those which he advocated in later years; opinions which he regarded as certainly just at one time were rejected as most probably incorrect after a few years of fresh labour; and whereas in 1785 he enunciated with some definiteness a theory respecting the constitution of the universe, he not only abandoned this theory (implicitly) in 1811, not only gave up the very principles on which it had been based, but he did not consider himself in a position during any subsequent part of his career to state with the same degree of definiteness any theory whatever respecting the constitution of the heavens.

It is to be premised, however, that even the theory of 1785[1] is not properly described in our text-books of astronomy, and that some of the accounts which purport to be descriptions are the merest travesties of the noble conceptions of Sir W. Herschel.

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[1] It will be observed that I pass over the paper of the year 1784, in which Herschel first began to discuss the constitution of the heavens. That paper is so obviously a mere introduction of his subject, so obviously preliminary, that one cannot but be amazed to find how much stress is laid upon it in many works on astronomy, and especially among French writers. We doubtless owe this to Arago's misapprehension of the purport of the paper. Struve has pointed out, also, very justly, that Arago has miscontrued the details of Herschel's theories. There scarcely exists, indeed, a less trustworthy account of Herschel's works and theories than that given by Arago, whom yet many English writers accept as an authority on the subject.

The sole point in the remarkable paper of 1785 which seems in any degree to correspond with the account usually given, is the hypothesis on which the principle of star-gauging is based. Herschel adopts a general uniformity of stellar distribution within the sidereal system as a means of forming some idea of the shape of that system. If there were actual uniformity, it would be sufficient to turn a telescope successively to different parts of the heavens, to obtain a sufficiently accurate idea of the extension of the stellar system in those different directions; for obviously, the farther away the boundary of the system might lie in any direction, the more stars would be seen in that direction. But though Herschel thus adopts a principle resembling that usually associated with the method of star-gauging, and though he arrives at a conclusion in some sense resembling that which has received the name of the Grindstone Theory (though it would more properly be called the Cloven Disc Theory), yet it is only on the most cursory reading that either the principle of generally equable distribution, or the conclusion that our sidereal system forms a cloven stratum of stars, can be regarded as according with Herschel's real views. He fully recognised the existence of subordinate systems within our sidereal system. This is shown by his remark that it is 'a very extensive, branching, compound *congeries* of many millions of stars, which most probably owes its origin to many remarkably large as well as pretty closely scattered small stars that may have drawn together the rest.'

Again, let the reader carefully study the following extract from the paper of 1785, and he will find that, whereas it is perplexing in the extreme if the sidereal system be regarded as a mere cloven stratum of stars, pretty uniformly distributed, it becomes perfectly clear (and wonderfully striking) when we remember that Herschel considered the Milky Way to be compound in structure and branching in figure:—

'If it were possible to distinguish between the parts of an indefinitely extended whole, the nebula we inhabit might be said to be one that has fewer marks of profound antiquity upon it than the rest. To explain this idea, perhaps, more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach; and whoever reflects upon the numbers of ages that must have passed before some of the clusters could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum.'

Yet more strikingly opposed to the common conceptions of Herschel's earlier theory, is the following passage from the paper of 1785:—'Our system after numbers of ages may very possibly become divided so as to give rise to a stratum of two or three hundred nebulæ; for it would not be difficult to point to so many beginning or gathering clusters in it. This view of the subject throws a considerable light upon the appearance of that remarkable collection of many hundreds of nebulæ which are to be seen in what I have called the nebulous stratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decomposed small nebulæ of a large, reduced, former great one to approach nearer to us in the sides than in other parts. Nay, possibly, there might originally be another large joining branch, which in time became separated by the condensation of the stars; and this may be the reason of the little remaining breadth of our system in that very place; for the nebulæ of the stratum of Coma are brightest and most crowded just opposite our situation, or in the pole of our system. As soon as this idea was suggested,' he adds, 'I tried the opposite pole, where accordingly I have met with a great number of nebulæ, though under a much more scattered form.'

It will be quite obvious, I think, that Herschel entertained at this time ideas altogether different from those usually ascribed to him. In particular, it is to be noticed that he regarded the nebulæ in two aspects at this time. There were some which he held to be parts of a much larger nebula, possibly parts of our own Milky Way. Others he held to be external Milky Ways, and therefore themselves made up of subordinate nebulæ. At this stage of his career, however, he seems to have entertained no doubt as to the stellar character of any nebulæ; and it is to the ideas he thus early promulgated respecting the nebulæ, that we must ascribe the common but altogether erroneous opinion, that Herschel regarded all the nebulæ as external galaxies of suns. He never entertained so general a theory as this; but even the theory he did entertain in 1785 respecting some of the nebulæ was modified ere long as respects a very large proportion of those objects.

He had not changed his views, however, in 1789, when he compared the nebulæ of various orders to plants in various stages of growth. The following passage does not, as has been asserted, relate to his hypothesis of the development of nebulæ into stars, but to the various orders of stellar systems. 'This method of viewing the heavens,' he says, referring to the view that differences of antiquity may account for the different appearances of nebulæ, 'seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions in different flowering beds; and one advantage we may at least reap from it is, that we can, as it were, extend the range of our experience to an immense duration. For to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing whether we live to witness successively the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the whole plant passes in the course of its existence be brought at once to our view.'

But the time was approaching when Herschel was to form entirely new views respecting the constitution of the heavens. I pass over the paper of 1796—though if space permitted I might quote passages from it, suggesting in a very interesting manner the progression of Herschel's ideas—and turn to the paper of 1802. There can be no mistaking the import of the following passages from this important paper :—'The stars we consider as insulated,' says Herschel, 'are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced by a long inspection and continued examination of it that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.' . . . . 'On a very slight examination it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments.' . . . . 'The milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near their undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way.' . . . . 'We may indeed partly ascribe the increase both of brightness and compression,' in these clustering regions, 'to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, the space containing the clusters in stars must tend to a spherical form if the gradual increase of brightness is to be explained by the situation of the stars.'

It was in this paper that Herschel first put forward the hypothesis that many nebulæ are formed of some substance possessing 'the quality of a self-luminous milky luminosity,' and 'possibly at no great distance from us.' But the requirements of space render it advisable that I should leave untouched the whole of those considerations which Herschel adduced, in this paper and elsewhere, in favour of his nebular hypothesis.

That portion of Herschel's observing career which we have now reached may fairly be regarded as including the most important of his researches into the constitution of the heavens. His mental powers were now at their prime. He had acquired great experience. His circumstances were such that he could give his whole attention to scientific work. Moreover, he had seen the importance of being solely guided by known facts, since the theory which he had adopted more than a quarter of a century before (and almost in the beginning of his observations on the stars) had had to be abandoned.

Nine years of earnest labour passed before he again spoke at any length on the subject of the star-depths. It will be admitted that we should attach especial value to the announcements he then made. Yet the striking words with which he prefaces the paper of 1811 seem almost to have escaped notice:—'I must freely confess,' he says, 'that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and, indeed, when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted should, on examination, prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance; but a longer experience and a better acquaintance with the nature of nebulæ will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed.'

In the paper of 1811, however, Herschel does not (save in these prefatory remarks) consider the structure of the sidereal heavens. The paper is devoted exclusively to the discussion of the nebular hypothesis as to the formation of stars. It was not until 1814 that he endeavoured to extend his reasoning so as to include the subject of stellar grouping. 'The observations contained in this paper,' he says in 1814, 'are intended to display the sidereal part of the heavens, and also to show the intimate connection between the two opposite extremes, one of which is the immensity of the widely diffused and seemingly chaotic nebulous matter; and the other, the highly complicated and most artificially constructed globular clusters of compressed stars.' In this paper he enters also into a discussion of ambiguous objects,—that is, objects 'of such a construction or at such a distance from us, that the highest power of penetration which hitherto has been applied to them, leaves it undecided whether they belong to the class of nebulæ or stars.'

But the chief interest of the paper of 1814 resides (in my judgment) in the remarks which Herschel makes respecting the clustering condition of parts of the heavens. He explains that his expression 'forming clusters,' was used 'to denote that some peculiar arrangement of stars in lines making different angles, directed to a certain aggregation of a few central stars, suggested the idea that they' (the former) 'might be in a state of progressive approach to them' (the latter). 'This tendency of clustering seems chiefly to be visible in places extremely rich in stars. In order, therefore, to investigate the existence of a clustering power, we may expect its effects to be most visible in and near the Milky Way.' I invite the reader's special attention to the circumstance that the Milky Way is here pointedly referred to as a stellar region distinct in its characteristics from the region of stars forming our constellations. In studying Herschel's papers we have to be continually on the watch for indications of the sort, since he does not always judge it necessary to make definite assertions of his opinions on such points.

He then describes the irregular clusterings of stars, noting in particular, that 'though they are in general very promiscuously scattered, they are yet sufficiently drawn together to show that they form separate groups,' while in many places a falling off in the number of stars surrounding the clusters 'indicates a tendency to future insulation.' 'Those which are in and very near the Milky Way,' he says, 'may be looked upon as so many portions of the great mass drawn together by the action of a clustering power, of which they tend to prove the existence.' In describing the various orders of irregular clusters, Herschel is particular to notice *rows* and *streams*, *ridges* and *shelvings*, of stars, as indications of a preponderating clustering power.

It is most important to notice here that Herschel had not yet replaced the theory of 1785 by any complete new theory.[1] He says at the close of the paper of 1814—'The extended views I have taken in this and my former papers of the various parts that enter into the construction of the heavens, have prepared the way for a final investigation of the universal arrangement of all these celestial bodies in space.' He was now in his *seventy-sixth* year, however, and though still in the full possession of his wonderful powers, the time that remained to him for investigating the universal arrangement of the heavens was short indeed, when the wideness of the subject is considered. But he was not even yet fully prepared to enter on the work. For he proceeds to mention as a reason for not doing so, that he is 'still engaged in a series of observations for ascertaining a scale whereby the extent of the universe, as far as it is possible for us to penetrate into space, may be fathomed.'

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[1] The careful Struve did not fail to recognise the distinction between the theory of 1785 and Herschel's subsequent labours. 'On peut demander,' he says, 'pourquoi les astronomes ont-ils maintenu généralement l'ancien système sur la Voie Lactée, énoncé en 1785, quoiqu'il eût été entierement abandonné par l'auteur lui-même comme nous l'avons démontré. Je crois qu'il faut en chercher l'explication dans deux circonstances. C'était un système entier, imposant par la hardiesse et la précision géométrique de sa construction, et que l'auteur n'avait jamais révoqué dans sa totalité. Dans ses traités publiés depuis 1802, on ne rencontre que des vues partielles, mais qui suffisent, en les comparant entre elles, à comprendre l'idée finale du grand astronome.' I should, point out, however, that Struve somewhat over-estimates the precision of the theory of 1785, and that he also fails to notice the circumstance that the papers of 1817 and 1818 are distinct in all respects from Herschel's former work.

In the papers of 1817 and 1818, the last of the series, and written, be it remembered, when Herschel was in his seventy-eighth and seventy-ninth years, he describes the principle here referred to, and its application to estimate the profundities of various parts of the stellar heavens. The principle was essentially this—that the telescopic powers necessary to reveal and to resolve star groups, or particular star orders within groups, afford an indication of the distance at which those groups or orders lie. I conceive that no question can exist that the principle is unsound, and that Herschel would himself have abandoned it, had he tested it earlier in his observing career, and when, *not* his mental power, *but* his mental elasticity, was greater than at the advanced age which he had now reached. I have not here space to enter into all the objections which present themselves against it. Presently, in considering Sir John Herschel's theoretical views, I shall have to adduce a conclusive instance of the unsoundness of the principle. Let it here suffice to remark, that repeatedly in applying it, Sir W. Herschel found regions of the heavens very limited in extent, where the brighter stars (clustered like the fainter) were easily resolved with low powers, but where his largest telescopes could not resolve the faintest. These regions, if the principle were true, must be long spike-shaped star groups, whose length is directed exactly towards the astronomer on earth,—an utterly incredible arrangement, even if we could believe in the dynamical possibility of such grouping.

Herschel did not live to carry out that final investigation of the universal arrangement of all the celestial bodies in space which he had looked forward to in 1814. In 1820 he read, as first President of the Astronomical Society, a paper on the double stars, and soon after his health broke down, insomuch that, though he lived till 1824, he was incapable of great or prolonged mental exertion.

Sir John Herschel's labours among stars and nebulæ were directed almost wholly to the completion of the survey of the heavens. I do not know that there is a single passage in his works in which he touches on the subject of the constitution of the heavens otherwise than as it were incidentally. He nowhere definitely enunciates a theory of the universe.

Two circumstances, however, in Sir John Herschel's work require to be attended to.

The first relates to the distribution of the brighter orders of stars over the heavens. In his 'Outlines of Astronomy,' he says that the stars of these orders, including those down to, and those somewhat below, the range of the naked eye, increase largely in number as the Milky Way is approached. In his 'Observations at the South Cape,' he asserts the reverse. 'On a general view it appears,' he says, 'that the tendency to greater frequency, or the increase of density in respect of statistical distribution in approaching the Milky Way, is quite imperceptible among stars of a higher magnitude than the 8th, and except on the very verge of the Milky Way itself, stars of the 8th magnitude can hardly be said to participate in the general law of increase. For the 9th and 10th the increase, though unequivocally indicated over a zone extending at least 30° on either side of the Milky Way, is by no means striking. It is with the 11th magnitude that it first becomes conspicuous, though still of small amount when compared with that which prevails among the mass of stars of magnitudes inferior to the 11th, which constitute 16-17ths of the totality of stars within 30° on either side of the galactic circle.' He then adds as a conclusion, 'following inevitably from this' (and this conclusion is to be carefully noted, since, if just, it is of the utmost importance), 'that the larger stars are really nearer to us taken *en masse*, and without denying individual exceptions, than the smaller ones. Were this not the case, were there really among the infinite multitude of stars constituting the remoter portions of the galaxy numerous individuals of extravagant size and brightness, as compared with the generality of those around them, so as to overcome the effect of distance and appear to us as large stars, the probability of their occurrence in any given region would increase with the total apparent density of stars in that region, and would result in a preponderance of considerable stars in the Milky Way, beyond what the heavens really present, over its whole circumference.'

The discrepancy between the statements in the 'Outlines' and in the 'Observations' is so complete that I ventured to communicate with Sir John Herschel respecting it, though I was not without a tolerably definite idea as to its origin. He replied as follows:—'I thank you for calling attention to that section in my "Outlines." Undoubtedly there *is* a discordance of statements which requires correction. But the appeal there' (that is, in the 'Outlines') 'is rather to a vague naked-eye impression than to the statistical result of actual enumeration; and assuredly on a cursory view of the heavens on a clear night, stars down to the 7th and 8th magnitude do affect the eye, though we cannot fix them by reason of that strange law which curtails a star *directly* looked at of a very large aliquot part of its photometric effectiveness.' It will be seen that Sir John Herschel here maintains at once the reality of the aggregation of the brighter stars on the *visible* Milky Way, and the justice of the statistics which related to the Milky Way regarded as a *zone*. This is undoubtedly the true explanation of the matter, as my equal-surface chartings have since abundantly demonstrated. The brighter orders of stars, down to the 11th, do unquestionably crowd upon the real Milky Way,—that altogether irregular region of star-streams and star-clusterings which has been aptly compared to a band of broken *cirrus* clouds; but if we lose sight of these irregularities, if we conceive of the Milky Way as of a uniform zone, and take the average distribution of the brighter stars over that zone, we no longer find that exceptional degree of richness, for the gaps and *lacunæ* of the Milky Way are as poverty-stricken, notwithstanding their position in that imagined *zone* of richness, as the brighter parts are rich in respect of these higher orders of stars. But this explanation shows that the conclusion based by Sir John Herschel on the zone-statistics should be *not merely abandoned but replaced by its converse.* Let the reader carefully re-study it in the light of the above considerations, and he will see at once that this is the case.

The second point to which I would invite attention in Sir John Herschel's work is his discussion of the phenomena presented by the two Magellanic clouds. His reasoning cannot be too carefully considered. It runs thus:—'Taking the apparent semi-diameter of the Nubecula Major as three degrees, and regarding its solid form as, roughly speaking, spherical, its nearest and most remote parts differ in their distance from us by little more than a tenth part of our distance from its centre. The brightness of objects situated in its nearest portions, therefore, cannot be *much* exaggerated, nor that of its remoter *much* enfeebled, by their difference of distance; yet within this globular space we have collected upwards of 600 stars of the 7th, 8th, 9th, and 10th magnitudes, nearly 300 nebulæ, and globular and other clusters *of all degrees of resolvability*, and smaller scattered stars innumerable of every inferior magnitude, from the 10th to such as by their multitude and minuteness constitute irresolvable nebulosity, extending over tracts of many square degrees. Were there but one such object, it might be maintained without utter improbability that its apparent sphericity is only an effect of foreshortening, and that in reality a much greater proportional difference of distance between its nearer and more remote parts exists. But such an adjustment, improbable enough in one case, must be rejected as too much so for fair argument in two cases. *It must therefore be taken as a demonstrated fact, that stars of the 7th and 8th magnitude and irresolvable nebulæ may co-exist within limits of distance, not differing more in proportion than as nine to ten.*'

I pass to the work of the elder Struve, but shall only deal with that portion which relates to the distribution of the stars.

Having found reason to believe that stars of the brighter orders are more crowded on the Milky Way zone than elsewhere, Struve tested the matter by comparing the numbers of stars in different hours of right ascension in Weisse's 'Catalogue' of 31,085 stars, down to the 9th magnitude, included between 15° north and 15° south of the equator. After estimating the numbers of stars which might be supposed to have escaped recognition in the different 'hours,' and so raising the total number of stars to 52,199 (of which 21,114 were hypothetical), he found a marked excess of richness in the Milky Way 'hours.' Thus, taking the four hours, 5, 6, 7, and 8 (crossed centrally by the Milky Way near Orion), and the four hours, 17, 18, 19, and 20 (crossed centrally by the Milky Way near Aquila), he found an average of 3,031 stars for each hour; while the average for the remaining sixteen hours was but 1,747 stars.

So far Struve's procedure was legitimate enough. But he now extended his reasoning somewhat too daringly. For he first regarded all the stars in each hour division as gathered on the equator, though the divisions extended *thirty* degrees in declination, and having thus a fine equatorial ring of 52,199 stars unequally rich in different parts, he conceived this ring converted into a flat disc by a radial spreading of the stars towards the several parts of the ring, this spreading being uniform between the distances corresponding (according to Struve's estimate) to the position of stars of magnitudes 1 to 6, of 7th magnitude, of 8th, and lastly of 9th magnitude. It seems impossible to attach the slightest weight to the evidence afforded by the resulting disc, since the construction of the disc is based on suppositions (as to the uniform scattering of stars radially towards the equator) which are directly contrary to Struve's own discovery of a want of uniformity circularly round the equator.

Struve deduced from this imperfect method of reasoning, combined with the study of Sir W. Herschel's star-gauges and a theory on the extinction of light, the conclusion that the Milky Way is unfathomable in its own level. He considered this view to be in accordance with the results obtained by Sir W. Herschel in his papers of 1817 and 1818. Herschel did not, however, find the Milky Way unfathomable all round the central line of the zone, but only in certain definite directions—towards the heart, in fact, of certain clustering aggregations. Struve misunderstood Herschel's remark that 'when his gauges would no longer resolve the Milky Way into stars, it was not because its nature is ambiguous, but because it is fathomless,' for he thus renders Herschel's words into French, 'nous pouvons faire là conclusion que *si* nos jauges cessent de résondre la Voi Lactée en étoiles, ce n'est point parce que la nature en est douteuse, mais parce qu'elle est insondable.' Doubtless he read Herschel's 'when' as equivalent to the German 'wenn.'[1]

As Sir John Herschel has remarked, disposing thereby at once of Struve's infinite extension theory and of his reason ing respecting the extinction of light, 'We are not at liberty to argue that at one part of the galaxy's circumference our view is limited as by a sort of cosmical veil, which extinguishes the smaller magnitudes, cuts off the nebulous light of distant masses, and closes our view in impenetrable darkness; while at another we are compelled, by the clearest evidence telescopes can afford, to believe that star-strown vistas *lie open*, exhausting their powers and stretching out beyond their utmost reach, as is proved by that very phenomenon which the existence of such a veil would render impossible, viz., infinite increase of number and diminution of magnitude, terminating in complete irresolvable nebulosity.' This reasoning is complete against Struve's inferences as based on Struve's assumptions. It is not, however, the less certain that irresolvable nebulosity does *not* necessarily imply that the objects producing it 'stretch out beyond the utmost reach of the telescope,' as is proved by the irresolvable nebulosity discovered by Sir John Herschel himself within the bounds of the Magellanic Clouds.

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[1] Thus is explained what Sir John Herschel and Professor Nichol clearly found perplexing, viz., Struve's conviction that the elder Herschel's results confirmed his own remarkable theory.

I shall treat very briefly of the results to which I have been led by my own researches into the subject of the constitution of the heavens, because they have been fully presented elsewhere.[1]

The purposes I have had in view throughout my inquiries have been two—First, to proceed, in perfect independence of all preconceived theories, to inquire how the stars and nebulæ are spread in space, how they differ in magnitude or constitution, and what laws govern their changes and movements, instead of adopting any assumptions on these points as bases for reasoning; and secondly, to endeavour in every case to render palpable to the eye the relations which have hitherto been presented only in elaborate catalogues or in tables of statistics. It is to subserve the latter purpose, which is obviously associated most intimately with the former, that I have adopted the method of equal-surface charting. In dealing with the proper motions of the stars I have drawn from each star an arrow whose length and direction indicate the rate and direction of the star's proper motion. I venture to believe that by these and other contrivances I have been enabled to exhibit the above-mentioned relations altogether more intelligibly than has hitherto been the case, and that laws hitherto unrecognised have thus been brought to light.

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[1] I may point out that the researches themselves, as originally submitted to the Royal Astronomical Society, are contained in my 'Essays on Astronomy.'

In the first place, I have been able to show that the stars, down even to the sixth magnitude only, are markedly crowded upon the Milky Way, and that, moreover, there are two rich stellar regions (as respects these lucid stars), nearly opposite each other, one covering the constellations Cepheus, Cygnus, Cassiopeia, and Draco, the other occupying the space included between the keel of Argo, Crux, and the Greater Dog. The following table (shortened from one in my 'Essays') indicates the numerical relations involved, the numbers in the second column representing the total number which would be visible if the whole heavens were as richly spread as the several regions indicated in the first column:—

	Richness.
Northern Milky Way	9,940
„ rich region	9,050
„ poor region (equally large) . .	2,567
Gaps in Milky Way	1,240
Southern poor region	2,361
„ rich region (equally large) . .	13,126
„ Milky Way	13,596

It will be observed that the richness of the whole Milky Way would be represented by 11,768, *exceeding more than nine times* the number representing the distribution of stars over the gaps and lacunæ of the Milky Way.

By extending the process of charting to include all the 324,198 stars in Argelander's series of forty charts, even more decisive evidence is obtained of the absolute intermixture in space—*i.e.*, within certain definite clustering aggregations — of stars of the brighter orders and the telescopic stars studied by the Herschels. For in the very regions where the Herschelian gauges showed the minutest telescopic stars to be most crowded, my chart of 324,198 stars shows the stars of the higher orders (down only to the 11th magnitude) to be so crowded that by their mere aggregation within the mass they show the Milky Way with all its streams and clusterings. This evidence, I venture to affirm, is altogether decisive as to the main question, whether large and small stars are really intermixed in many regions of space, or whether the small stars are excessively remote. It is utterly impossible that excessively remote stars could seem to be clustered exactly where relatively near stars are richly spread. This might happen, no doubt, in a single instance, but that it could be repeated over and over again, so as to account for all the complicated features seen in my chart of 324,198 stars, I maintain to be utterly incredible. (See Plates XI. and XII.)

By applying the process of equal-surface charting to the nebulæ, I find that they are so arranged over the heavens as to leave no room for doubting their association with the sidereal system. Moreover, they are found to run into streams or branches, intimately associated with streams of stars. And strangely enough, as the great star-streams seen in Eridanus and Aquarius are prolonged so as to enclose the Magellanic Clouds, so the great southern nebular streams extend themselves to the same groups. The association of these streams of nebulæ and stars could scarcely be regarded as accidental; but the fact that both sets of streams converge upon those very regions where stars and nebulæ are so strangely intermingled, removes all doubt from the conclusion that in the nebulæ (for these streams include all the known southern nebulæ) we have not to deal with external galaxies, but with objects belonging to the sidereal system.

The charting of the stellar proper motions has further led me to the discovery of the fact that the stars in many parts of the heavens are travelling in systems—or as it were drifting—through space. Thus the five stars, β, γ, δ, ε, and ζ, Ursæ Majoris, are found to be all moving together, as one grand scheme.[1] In Gemini there is a much more remarkable instance of drift, nearly all the stars of that constellation (Pollux is one of the exceptions) travelling in one general direction. In Taurus there is also well-marked drift. These instances of drift suffice to afford independent testimony in favour of the existence of systems subordinate to the great sidereal system.

The general conclusions to which I have been led by these and other methods of research, which space will not here permit me to particularise further, are chiefly these:—The sidereal system is altogether more complicated, altogether more varied in structure, than has hitherto been supposed. Within one and the same region co-exist stars of many orders of real magnitude, the greatest being thousands of times larger than the least. All the nebulæ hitherto discovered, whether gaseous or stellar, irregular, planetary, ring-formed, or elliptic, exist within the limits of the sidereal system. They all form part and parcel of that wonderful system whose nearer and brighter parts constitute the glories of our nocturnal heavens.

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[1] More than two years ago, speaking of this discovery, which I had then but recently made, I expresssed my conviction that if ever Dr. Huggins was enabled to determine the proper motions of these stars in the line of sight, he would find them to be all travelling one way, either receding or approaching. Quite recently he has tested the matter, and he finds that not only have these five stars similar spectra, unlike the spectra of α and η, but that whereas α is approaching, and η receding more slowly, the five are all receding at the rate of twenty miles per second.

It has been supposed that the new views to which I have been led would, if accepted, tend to reduce our estimate of the scale on which the universe is constructed. This, however, is not the case. It is true that I cannot recognise in the clustering regions of the Milky Way the profundities commonly believed in; but I believe in profundities far vaster. It is true that I believe the varieties of structure brought into view by higher and higher telescopic powers to be real and not apparent; but if I thus recognise the existence of stars much smaller than neighbouring orbs, it is because I recognise in the larger orbs suns which surpass our own thousands of times in volume; while this very variety enhances our conceptions of the wonders of the sidereal system. It is true, again, that I cannot recognise in the star-cloudlets the external galaxies spoken of in our text-books of astronomy, that I recognise parts of our sidereal system where hitherto the common opinion has been that outlying universes are in question; but I reason thus, because I have been led to the conclusion that our sidereal system is much more extensive than has hitherto been supposed. I do not draw the nebulæ inwards to the star-depths, but I extend the star-depths outwards until they include the nebulæ. According to my views, the range of our telescopes is neither greater nor less than has been hitherto supposed: if, then, I conceive that the sidereal system reaches to unknown depths beyond that range, it is not because I would reduce our estimate of the scale on which the sidereal system is constructed, but because I would enlarge that estimate, and that not by a little but indefinitely.

In concluding, I would say a few words on the wonderful scene presented by the star-depths. Let our thoughts pass from our Earth, which seems so magnificent, to the giant orbs of Saturn and Jupiter, which dwarf her dimensions to insignificance; thence to the Sun, compared with which the largest of the planets seem so small. Then let us consider the dimensions of the solar system, compared with which even the dimensions of the Sun are as nothing. Next let us pass on in thought to the vast région of space within which our whole solar family is travelling. Then let us picture the scheme of suns of which our Sun is a member;—not the sidereal system, scarcely even an appreciable fraction of that system, but the particular family of suns to which the Sun belongs—and let us consider how the domain of the Sun, the region of space over which he bears sway, is in its turn reduced to mere nothingness by comparison with the scheme of suns of which our Sun is a member. Then, lastly, let us picture to ourselves that the scheme of stars to which our Sun belongs is but one of the atoms of which the frame of the sidereal systems is built.

We can speak of these things, but we cannot conceive them. The astronomer can spread out the figures which represent these wonders, but he can neither enable others to conceive them, nor can he conceive them himself. I know not, then, how I can more fitly draw my subject to a conclusion than by quoting the wonderful dream in which the German poet, Jean Paul (nobly translated by our own prose poet, De Quincey), pictures the feebleness of human conceptions in the presence of the infinite wonders of the universe:—

'God called up from dreams a man into the vestibule of heaven, saying, "Come thou hither, and see the glory of my house." And to the angels which stood around His throne He said, "Take him, strip from him his robes of flesh; cleanse his vision, and put a new breath into his nostrils, only touch not with any change his human heart, the heart that weeps and trembles." It was done: and with a mighty angel for his guide the man stood ready for his infinite voyage; and from the terraces of heaven, without sound or farewell, at once they wheeled away into endless space. Sometimes with the solemn flight of angel wings they passed through Zaharas of darkness, through wilderness of death, that divided the worlds of life; sometimes they swept over frontiers that were quickening under prophetic motions from God. Then from a distance which is counted only in heaven, light dawned for a time through a shapeless film; by unutterable pace the light swept to them, they by unutterable pace to the light. In a moment the rushing of planets was upon them; in a moment the blazing of suns was around them. Then came eternities of twilight, that revealed but were not revealed. On the right hand and on the left toward mighty constellations, that by self-repetitions and answers from afar, that by counter-positions, built up triumphal gates, whose architraves, whose archways, horizontal, upright, rested, rose, at altitude, by spans, that seemed ghostly from infinitude. Without measure were the architraves, past number were the archways, beyond memory the gates. Within were stairs that scaled the eternities around; above was below and below was above, to the man stripped of gravitating body; depth was swallowed up in height insurmountable, height was swallowed up in depth unfathomable. Suddenly, as thus they rode from infinite to infinite, suddenly, as thus they tilted over abysmal worlds, a mighty cry arose that systems more mysterious, that worlds more billowy, other heights and other depths, were coming, were nearing, were at hand.

'Then the man sighed and stopped, shuddered and wept. His overladen heart uttered itself in tears, and he said, "Angel, I will go no farther; for the spirit of man acheth with this infinity. Insufferable is the glory of God. Let me lie down in the grave, and hide me from the persecution of the Infinite, for end I see there is none." And from all the listening stars that shone around issued a choral voice, "The man speaks truly; end there is none that ever yet we heard of!" "End is there none?" the angel solemnly demanded; "Is there indeed no end? And is this the sorrow that kills you?" But no voice answered, that he might answer himself. Then the angel threw up his glorious hands to the heaven of heavens saying, "End is there none to the universe of God. Lo! also, there is no beginning."'

From the *Quarterly Journal of Science* for July 1872.

NOTES ON STAR-GAUGING.

As several telescopists have expressed a wish to take part in the work of star-gauging, and to be informed as to the best way of making their work effective, I venture to give a few hints on the subject.

In the first place, it is to be noted that the main object of the proposed method of observation is to obtain complete general surveys of the heavens with various apertures. It is the direct result of the researches which I have already made, that mere random star-gauging will not suffice to give just views of the structure of the sidereal system; that, on the contrary, such a process is more likely to prove deceptive than useful. What is wanted is a survey which shall include the whole heavens (eventually), or at the beginning shall include the whole of each region surveyed, without gaps or interstices. But the extent of such a survey renders it absolutely necessary that it should be conducted in a manner the least elaborate that can be devised, so only that no essential details be neglected. To *map* the whole heavens with telescopes of considerable light-gathering power would require an army of observers, while only a few volunteers are available; moreover, such mapping, while having a high value in other ways, would be less instructive regarded as star-gauging than a survey conducted by a few observers, each taking a considerable area. For the different powers of the observers engaged would cause noteworthy differences in the apparent numerical distribution of stars, and such apparent differences would be confounded with the real varieties of distribution which it is the object of star-gauging to discover.

As respects the first point, completeness, I may remark that I have obtained very striking evidence on that subject. For the winter before last, with the $4\frac{7}{10}$-inch Sheepshanks belonging to the Society, I surveyed the region of the heavens included in the parts of *Taurus* north of 15° North Dec., using circular gauge-fields, touching each other in R.A. Now, the results of the gauging thus conducted were so far interesting that they indicated in a very marked manner the prolongation (with increase of telescopic range) of that remarkable region poorly strewn with stars, which is shown in this part of the heavens in my chart of 324,000 stars. (See Plate XII.) But on commencing the re-examination of this part of the heavens with the same telescope and eyepiece, but a field reduced to the figure of a square, I found a marked difference in the result as respects many details, though the general distribution was not very different.[1]

As respects the method to be employed, I think the following notes may be useful :—

A square field is the most convenient, and the side of the square may conveniently correspond to 15′ of a great circle. But this is not important—the field need not have any special size. An edge of the square should of course lie on a parallel of declination. The gauging eyepiece should be of rather low power, but not too low, especially with large instruments. The most suitable power will be indicated by what I have said as to the size of the field, if the diaphragm used to reduce the field be such as to give a square not much less than the inscribed square within the original circular field. On this point, however, little need be said, as in fact each observer can consider his own convenience, and so long as the nature of the eyepiece and size of field are indicated in the gauge-book (together, of course, with the aperture of telescope, &c.), no difficulty will arise.

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[1] I had proposed to resume the survey with the same telescope (using square fields) this year. But having had one of Mr. Browning's $12\frac{1}{4}$-inch reflectors placed at my disposal by Lord Lindsay, I naturally prefer to devote my time to a deeper survey.

The most important point is the way of taking and recording gauges. The work must be done in the dark, except when a new sweep is to be commenced. Therefore the observer should provide a number of squares of paper (or preferably card) to be strung on a cord, and mark in, in pencil, the number of stars counted in each field, passing each paper when thus marked from the set of unused papers to those already marked.

When a sweep begins, the telescope (clamped of course in declination) is directed to the western extremity of the proposed sweep. The number of stars in the field is counted rapidly, and the telescope is moved eastwards until the western edge of the new field corresponds (as nearly as can be judged by estimation) with what had been the eastern edge of the former field. The process is repeated, field after field, until the proposed range of sweep has been completed (so nearly as can be judged). Thus, say the width of each field is 20′ in R.A., and that the observer desires to range 15° in R.A., he would count in this way 45 fields (or say two or three more to ensure the completion of the desired range). He must now determine the exact distance he has swept over, either in the regular way, when he has a suitable clock, or by noting the apparent difference of R.A., as indicated by the R.A. circle, adding so much as corresponds to the time occupied in sweeping. The necessity for this process is obvious, when it is remembered that the shift from field to field has been made by estimation only, and *can* only be so made, except by methods which would involve delay and inconvenience. Thus the fields may slightly overlap, or there may be slight gaps (though no star can escape being counted), and the real range in R.A. may be several minutes less or greater than would be judged from the number of fields. But knowing the exact range of the sweep, and in the gauge maps (afterwards to be constructed) dividing that range into as many equal parts as there were fields, no error of the least importance can accrue. The numbered papers should then be removed, with a portion of the string, this being tied round them, so that these papers may remain unchanged in order. The telescope must then be returned to its former position in R.A., that is the first field brought again into view, in doing which the range of R.A. can again be noted, if any doubt be felt as to the former determination. Then the telescope must be shifted in declination until a field north or south of the first is in view. This will, of course, be done in the usual way, by means of the declination-circle, since the exact size of the field is known. Another sweep will then be taken in the same way as the former. And the process may be repeated as long as the observer cares to remain at work. Care must, of course, be taken to keep the successive strings of gauge papers distinct. This is readily effected by simply marking the first paper on each string with the declination of the central line of the sweep.

From the *Monthly Notices of the Astronomical Society* for June 1873.

PRINCIPLES OF STAR-GAUGING.

From communications which have reached me, I am disposed to believe that some misapprehensions probably exist respecting the nature and objects of the processes of star-gauging which I have suggested as forming at present the best available means for determining the laws according to which the stellar universe is constituted. I therefore add some remarks which may be of use in explaining my views.

First, the great object of star-gauging with any given instrument is to ascertain how many stars *that instrument* shows in each portion of the heavens, when the sky is clear and dark, and there is no twilight, moonlight, or other cause of variation in space-penetrating power. Great exactness in enumeration is by no means necessary. In fact, the effort to secure great exactness, would certainly, by extending the time necessary for the work of survey, defeat the whole object of the work. What is required is a complete but rapidly effected survey, bearing the same relation to the actual charting of stars, that the reconnaissance of a land region bears to trigonometrical survey.

In interpreting results, attention must be directed, *first*, to the numerical distribution indicated by each particular instrument, such distribution being presented to the eye by the process of equal-surface charting; and, *secondly*, to the differential results indicated when a survey by any given instrument is compared with a survey by another instrument of greater or less space-penetrating power.

Unless both these orders of indication are considered, the star-gauging can afford no satisfactory information. This will be manifest if it be remembered that according to my views,—and I think I may say, according to the results I have demonstrated by equal-surface charts already constructed,—the great difficulty in all researches into the construction of the heavens consists in distinguishing the effects of star-distribution resulting, on the one hand, from great extension in the line of sight with consequent great variation in the apparent magnitude of stars of the same order, and on the other hand from the aggregation within one and the same region of many orders of real magnitude. These, of course, are only the extreme cases, and in nearly every instance, if not absolutely in every instance, both causes—varying apparent magnitude and closeness of aggregation—are effective. It is *absolutely impossible in my opinion to distinguish one from the other, or to ascertain in what degree one or other operates, except by the proposed method of star-gauging, which combines, be it noticed, the qualities of the two methods suggested by Sir W. Herschel (one in* 1784, *the other in* 1817), *without being liable to the causes of error which exist in either of those methods. The general principles of interpretation, applicable not only to star-gaugings with different instruments, but to all differential observations whether relating to colour, to duplicity, triplicity, or grouping generally, to spectrum, to motions whether thwart or in the line of sight, to peculiarities of configuration, to the various orders of nebulæ, or so on, are simply these.*

(i.) *Where two orders of observation indicate concordant laws of distribution over the heavens, the rich regions so* *indicated are regions where the two orders of objects are intermingled.*

(ii.) *Where the results are in direct contrast (or discordant in the true sense of the word),*[1] *the rich regions for one order corresponding to the poor regions for the other, and* vice versâ, *the two orders of objects belong to one system, but some peculiarity in their nature or in the laws according to which they were formed, causes them to occupy different parts of the system, segregating as it were from each other.*

(iii.) *Where no connection whatever, either of agreement or contrast, can be observed, it is probable, and in general presumable, that the two orders are altogether distinct and lie at different distances from each other.*

(iv.) *And lastly, where partial or local agreement or contrast is indicated, then the inference is that the true arrangement of the objects in space is affected both by laws of aggregation or segregation, and by diversities of distance and by one cause or the other to a degree indicated by the extent of such agreement or contrast.*

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[1] Which, however, is ordinarily used in too vague a way to be suitable as the converse of concordant.

From the *Monthly Notices of the Astronomical Society* for October 1873.

SUMMARY AND CONCLUSION.

It appears to me desirable to conclude my discussion of the relations presented by the sidereal universe—so far as these pages are concerned—by indicating briefly but distinctly the points in which my views differ from the views advanced by those who have preceded me in the discussion of this subject.

I must premise that Sir John Herschel, the late Earl of Rosse, Whewell, and Herbert Spencer have advocated more or less decidedly some of the opinions which I have been led independently to enunciate. Regarded as a whole, however, my views are original, and several of the points to which I draw attention were first noted, I believe, by myself. If not, I would desire to be corrected, so as not to present as new views opinions which have already been advocated by others.

In the first place, then, I lay down as a fundamental rule that no *hypothesis* as to star-magnitudes or star-distribution can safely be adopted as a basis of research. In ordinary subjects of inquiry, it is well to have working hypotheses, varying perhaps as we proceed, but serving conveniently for the co-ordination of observations. But in researches into the constitution of the stellar universe, we must not adopt any hypothesis until observations sufficiently numerous and extensive have indicated its justice.[1]

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[1] It is rather singular that my inquiries, *the first systematic inquiries ever based on observations as they stand,* without assumption of any sort, should have

It was owing to his failure to recognise this principle at the beginning of his work (though, I need hardly say, it is to his work that I owe the main evidence in favour of the principle) that Sir W. Herschel formed the stratum theory of the sidereal system, which French and English text-books of astronomy persist in describing as a demonstrated theory, though Herschel himself definitively abandoned it. It will be well to consider this case fully, partly because no opportunity must be lost to indicate the mistaken nature of the text-book accounts of Herschel's work, and partly because of the intrinsic importance of the discovery which Herschel really effected.

It is known that, in 1784, Sir W. Herschel, who then supposed that our sidereal system is continuous within its limits, and consists of stars strewn (numerically, not in respect of real magnitude) with a certain general uniformity throughout the scheme, suggested his plan of star-gauging. It is clear that, adopting these hypotheses, a telescope large enough to penetrate to the limits of the system in all directions, could be employed to determine the shape of the system. For, the number of stars in any given direction, and within a given field, would afford a measure of the extension of the system in that direction.

And manifestly, if we add to the two hypotheses just stated this further hypothesis—that the sidereal system has some tolerably regular figure, as spheroidal, disc-like (circular or oval), cloven discoid, or the like, then a considerable number of scattered gaugings would afford suffi-

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been so misunderstood by several as to be described as theoretical by comparison with such inquiries as the earlier work of Sir W. Herschel, based on assumptions which were not only questionable in themselves, but were rejected eventually by Herschel himself, or with such investigations as those of Struve, based, as Encke long since pointed out, on assumption upon assumption! Those who are acquainted with the literature of the subject have, however, avoided this mistake.

ciently accurate general evidence as to the true nature of such figure.

It was from the combination of these hypotheses and from a moderate number of gaugings that, in 1785, Sir W. Herschel derived the impression that the sidereal system is shaped like a cloven flat disc, having its greatest extension towards the Milky Way, all whose stars form part and parcel of the system and conform (according to the views of 1785) to the hypotheses adopted by Herschel.

In 1802, Herschel definitely expressed his abandonment of the hypothesis of generally equable distribution, and necessarily with that hypothesis he gave up the cloven-stratum theory of the sidereal universe, and the supposition that the stars of the Milky Way are distributed like those immediately surrounding us. The passages from the paper of 1802 (quoted at pp. 187–189) serve to indicate Herschel's change of views even to those not well acquainted with his earlier papers; but the real significance of these passages will only be appreciated by those who have thoroughly mastered his whole series of papers (*which, judging from my own experience, implies the careful perusal of the whole series at least three successive times, and the study of certain portions in the same way in which one would study some difficult mathematical proposition*).

Nine years of labour passed before Herschel again dealt with general considerations respecting the sidereal universe. The passage from the paper of 1811 (quoted at p. 189) deserves to be carefully studied by some who have considered it a sort of high treason in me to question the soundness of the cloven-stratum theory of the sidereal universe.

Another hypothesis adopted by Herschel, though it was not eventually abandoned like those on which his star-gauging was based, equally illustrates how unsafe is the adoption of hypotheses on inquiries directed to the subject of the constitution of the universe. I refer to that method of gauging which he based on the supposed space-penetrating powers of his telescopes. This method was suggested in 1817, when Herschel was in his seventy-eighth year, and some results of its application were discussed in 1818. It appears to me that, while the papers of 1817 and 1818 afford unquestionable evidence of *power*, they show equally clear traces of the failing elasticity of his wonderful mind. Having devised a working hypothesis, he works straight on with a diligence and effect worthy of his best days; but he does not look around for tests as he did in the days when his first method of star-gauging was in use by him. Or rather (for, in his earlier years, he did not so much search for tests as recognise those which were in effect applied as he worked), we do not find him quick to notice, in 1817 and 1818, how his results negatived his hypothesis.

I conceive that no one who considers the real significance of Herschel's results, in 1817 and 1818, can fail to admit that they are utterly inadmissible, and that they therefore show the theory on which they are based to be altogether untenable. Herschel had taken resolvability as a test of distance. But he found regions which, while partially resolvable with his lowest powers, where not wholly resolvable with his highest. These limited regions of the heavens indicated, according to his hypothesis, long stellar projections turned directly towards the Sun, appreciably cylindrical but really conical, with the Sun at their vertex—*a monstrous supposition, and one such as Sir W. Herschel had repeatedly rejected in his earlier years.* It was by reasoning justly on precisely such results that Sir John Herschel was led to regard the Magellanic Clouds as roughly spherical. Sir W. Herschel had arrived, as we have seen, at a precisely similar conclusion respecting the figure of the clustering aggregations of the Milky Way. Yet it was to some of these very aggregations that he now applied the new gauging test, with results thus irreconcilable with those he had before obtained. It was by applying this new method that he arrived at the conclusions, on which Struve afterwards so strongly insisted, that parts of the sidereal system are absolutely unfathomable. Yet his former and sound principle of interpretation, and the principle which Sir John Herschel, Whewell, and Herbert Spencer applied to the Magellanic Clouds, show that where (in extreme old age) Herschel *thought he was penetrating to the extreme limits of the sidereal system, he was in reality only analysing more and more searchingly an aggregation in which many orders of stars were mixed up. What he failed to do was, not (as he supposed) to sound the galaxy, but to recognise as separate stars the minutest orders of orbs included within such aggregations.*

Struve, strangely enough, supposed that he had made either no assumptions which could fairly be regarded as such, or else none but the safest assumptions. Forbes and Encke more justly weighed Struve's researches. His assumptions, indeed, were the most daring which perhaps have ever been applied to the subject we are upon. Encke enumerates five assumptions, all of them questionable. I shall note only two, both occurring in the process by which he constructed the famous *Section* of the sidereal system, about which some writers have written in terms which would be exaggerated if applied to the labours of the elder or the younger Herschel.[1] Having counted (or rather obtained directly from Weisse's numbered catalogue) the numbers of stars, down to the ninth magnitude, in the different hours of an equatorial zone *thirty degrees wide* (from + 15° to − 15°), he first assumes that the distribution there indicated may be regarded as true *for the equator itself;* and then, secondly, he assumes that the stars towards each hour of the equator may be distributed over the sector formed by radial lines from the Sun to the extremities of the hour-arc, according to certain assumptions as to the relative distances of the various orders of star-magnitude. The combination of the twenty-four sectors constructed on these assumptions (which are not only not supported by evidence, but strongly opposed by all the evidence thus far obtained), forms Struve's equatorial star-disc. On this insecure evidence, and on a singular mistake as to the meaning of an expression employed by the elder Herschel,[1] Struve based his famous hypothesis of the extinction of light, his analysis of the proper motion of the Sun in magnitude, and other results equally interesting, and, in my judgment, equally unreliable.[2]

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[1] I wish it to be carefully noted that, throughout my inquiries into the constitution of the heavens, I have not said one word which has indicated other than a justly high estimate of the value of the Herschels' *labours*.

Sir John Herschel, so far as his own inquiries, as distinguished from those which he carried out in pursuance of his father's plans, were concerned, carefully avoided the adoption of any hypothesis, basing his opinions on observed facts solely. I must note, however, that he does not show the same acquaintance with his father's series of papers as Struve appears to have obtained. I think this is not altogether to be wondered at. Sir John Herschel took too large a part in original astronomical investigations to have leisure for that thorough investigation and analysis of his father's series of papers by which alone their real bearing can be ascertained. Be this as it may, the fact is patent that Sir W. Herschel abandoned the method of gauging which he had adopted in 1784, and that Sir John Herschel applied this method to the southern heavens. It is not wonderful that he obtained a result not according with that which his father had obtained in 1785, for it is the property of erroneous methods to lead to incongruous results. It was thus that the 'cloven flat-ring' theory was adopted by Sir John Herschel in preference to the 'cloven flat-disc' theory.

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[1] Herschel wrote that '*when* he could not resolve the Milky Way it was because it is unfathomable,' meaning clearly that certain parts of the Milky Way lay beyond the power of his strongest telescopes. Struve, probably, in making extracts in German, wrote 'wenn' for 'when;' at any rate, in the French of his 'Études Astronomie Stellaire,' we find the word 'si' for the English 'when,' and the whole meaning of the sentence is changed.

[2] I write this with some confidence. Sir J. Herschel has shown the weakness of the reasoning on which Struve based the theory of the extinction of light. As to the rate of the Sun's proper motion, I showed long ago the probability that this rate exceeds that assigned to it by Struve; and Dr. Huggins's recent researches into stellar motions of recess and approach may be regarded as conclusive on the subject.

But Sir J. Herschel's discussion of the peculiarities which he had recognised in the distribution of stars, his analysis of the features of the Milky Way, and his treatment of the evidence derived from the Magellanic Clouds, are characterised by sound and clear-sighted reasoning. We owe to him, in fact, the initiation of those processes and methods of inquiry which have more recently been applied.

Passing to my own treatment of the subject, I remark, first, that it has seemed to me very necessary to substitute for statistical research, at least in the beginning of inquiries into the laws of stellar distribution, some process not requiring preliminary hypotheses. I can conceive no general statistical process absolutely free from hypothetical considerations. Statistics can be satisfactorily applied to inquiries suggested by other and less deceptive processes; but at the beginning we cannot *count*, except in accordance with some pre-arranged plan, and such plan must necessarily be based on hypothesis. We see this in Struve's researches. He counted the number of stars in given hours of right ascension; but the result was meaningless, except on the assumption that the distribution of stars over a given hour, and between declination circles + 15° and − 15°, possessed a certain significance. We see, in fact, that he regarded the richness of distribution around the equator itself as indicated by the richness of star-distribution over areas thirty degrees in declination by fifteen degrees in right ascension; and unless that assumption, or some like assumption, were made, the mere numbers he obtained could not, so far as I can see, have any significance at all.

But then there is the risk in such a case that, in giving significance to statistical results by means of some hypothesis, we may be misled—the result only becoming significant by being made erroneous.

Charting, so long as it is isographic, seems open to no such objection. We plot down stars, or nebulæ, or variables, or red stars, or double stars, as the case may be, and accept the result, whatever it may be—certain at any rate that we shall not be taught anything erroneous, though the charting may not lead to any result of importance. Whatever there is to learn, we shall probably learn from a well-constructed chart; and we may learn much more than had been hoped when the chart was commenced, or else—and this has repeatedly been my own experience—a chart, while, not in itself teaching any noteworthy lesson, will suggest either statistical research or some new process of charting by which important new knowledge may be obtained.

From my equal-surface charts of the brighter orders of stars, I infer—I think I may even say I have demonstrated—that there is a much greater range of difference in real star-magnitudes than had been supposed. Unless the laws of probability are to be abandoned, those lucid stars which gather richly over the area of the Milky Way, are really immersed among the clustering aggregations of smaller stars which produce the milky light of the galaxy. In this case those brighter stars must be not only much larger, but *many hundreds of times* larger than the fainter stars of the galaxy.

We have here the first step towards just views of the constitution of the Milky Way, or rather the next step beyond the great, but little noticed, discovery of Sir W. Herschel's, that the bright clouds of the Milky Way are for the most part spherical clusters of stars. The excess in the number of lucid stars in the Milky Way proves that the stars of our constellations are among the leading orbs of the spherical aggregations. Plates I., II., and VI. serve to exhibit other peculiarities illustrated by the mapping of stars visible to the naked eye. Little is required by way of explanation of these charts, which are, as all such charts should be, self-explanatory. It is only necessary to mention that Plate II. exhibits the whole heavens on Flamsteed's projection (which is an equal-surface one); and although in parts the distortion is monstrous, yet as the distribution is always truly represented, the great purpose of star-mapping, so far as concerns our subject, is fully served. However, it would be a useful exercise to those having sufficient leisure, to make other similar charts on the same projection, but taking other meridians for the central line. Charts on the same projection, but on a larger scale, and including the nebulæ, as in Plate VIII., would also be highly instructive. I can, however, only indicate at present the desirability of such charts, my time being too fully occupied with other matters to enable me to carry out all the mapping processes which I should like to see applied. Before long, should I not be anticipated, while carrying out the processes of star-gauging which I have already commenced, I shall probably also obtain opportunities for constructing a complete series of charts illustrative of the laws of stellar and nebular distribution; but for the present those already drawn may be regarded as sufficient. Indeed it is not to be expected that others *applied to the same objects*, would reveal new relations of importance, though strengthening and elucidating the evidence given by those already drawn.

Charts including stars down to the seventh and eighth orders, yet remain to be constructed. I am not acquainted with any suitable materials. For the purpose of true star-gauging it is essential that the same uniform scale of star-magnitudes should be used; and as yet no complete catalogue or series of charts in which the stars of the seventh and eighth magnitudes have been estimated according to a uniform scale, are accessible, or at least known to me. I may remark, indeed, that charts alone would serve my purpose, since the labour of constructing equal-surface maps from complete catalogues including stars down to the eighth magnitude, would be enormous, and though the results would be interesting, they would probably scarcely repay the labour.

The existence of Argelander's series of charts of stars visible in the northern heavens with a telescope 2¾ inches in aperture, alone rendered my next step—the construction of an equal-surface chart containing all such stars—a feasible, though not altogether an easy one. I must remark, that the charts of Argelander's series cannot be regarded as quite exactly representing the distribution of stars over the whole of the northern heavens. Anyone who will test them systematically with the telescope, will soon recognise the fact that many stars well within the range of a telescope 2¾ inches in aperture have been omitted. This, of course, was to be expected. But it is remarkable that in many cases stars have been noted which are certainly not ordinarily to be seen with Argelander's selected small telescopic power.

Nevertheless, regarded as a whole, the series of charts constructed under Argelander's superintendence represents fairly the distribution of stars down to the eleventh magnitude of Sir J. Herschel's scale. The inexactnesses relate to minutiæ, not to the broad features which can alone be dealt with in the present stage of the inquiry.

I must remark here, that although in my equal-surface chart including Argelander's series, I have marked down all the stars, as nearly as the eye could judge, in their true place within the spaces (one degree each way in R. A. and Decl.) to which they severally belong, I have by no means done this with the intention of constructing a chart which could be used for reference to individual stars. In point of fact, I wanted only to ascertain the general distribution of the stars, and my purpose would have been equally, or nearly, as well fulfilled, if in each space I had marked down *at random* the proper *number* of stars. But, strange though it may seem at a first view, it was *easier* for me to make an exact transcript for each space than merely to jot down so many stars. The reason is, that by copying from each space, I had at once a means of distinguishing one space from another in my chart as compared with the original. Nothing could have been more baffling than the attempt to preserve merely the numerical distribution, and of course the result was somewhat more accurate, even as regards the indication of laws of aggregation.

The significance of the results obtained by this second step—that is, the step from stars visible to the naked eye, to stars visible with 2¾ inches of aperture—can only be appreciated by those who have carefully followed, and thoroughly mastered, the reasoning of the elder and younger Herschel, and of the elder Struve. I assert, without the slightest fear of contradiction by any possessing such knowledge, that the broad teaching of the equal-surface chart of 324,000 stars disposes finally of all theories of the constitution of the sidereal universe which had previously been enunciated. The chart does not definitively indicate a new theory—rather it suggests the idea that the constitution of the sidereal universe is too complex to be at present ascertained. But it completely negatives (i.) the stratum theory (even in the modified form apparently retained by Sir W. Herschel); (ii.) the flat-ring theory of Sir John Herschel; and (iii.) the infinitely extended stratum theory, with condensation towards the mean plane, which Struve adopted.

The force of the evidence derived from the chart consists in the position which the gauging power of Argelander (for it is to his gauging power, not to the refinements of his star-charting, that I alone appeal) bears to naked eye vision on the one hand, and to the powers used by the elder Herschel on the other. Powers intermediate to Argelander's and Herschel's may give important evidence, and doubtless will, when applied in accordance with the plan I have elsewhere suggested; but such evidence cannot have the value which resides in Argelander's work. A much lower telescopic power than Argelander's would bring the range too near that of ordinary vision; a much higher power would bring the range too near that of the Herschelian gauging telescopes. It is the intermediate character of Argelander's telescopic survey which constitutes its real value.

This I propose briefly to indicate.

The eye shows the great Milky Way stream, with its irregularities, branching extensions, nodules, and other peculiarities. The eye also indicates (in combination with suitable processes of charting and enumeration) the gathering of lucid stars on the Milky Way. Such is the evidence at one extremity of the scale. At the other we have the result of the Herschelian star-gauges, and especially of those gauges, not tabulated, which both the Herschels applied to rich galactic regions; and we learn from it, according to the words I have already quoted, that in Herschel's sweeps of the heavens 'it has been fully ascertained that the brightness of the Milky Way arises only from stars, and that their compression increases according to the brightness of the Milky Way.' This relates, as we know, to the stars brought into view by his great telescopes. Nothing, then, can be more interesting and important in connexion with the subject of the sidereal universe than the question whether the naked-eye brightness, and the gathering of naked-eye stars on the one hand, and the great richness of stars of the faintest orders reached by our telescopes on the other, are accompanied by an exceptional richness of stars brought into view by an intermediate power, far enough removed from naked-eye vision on the one hand to give those large numbers which raise probabilities into certainties, and far enough removed from Herschel's space-penetrating power on the other hand to ensure a real selection of larger stars from among the practically infinite numbers of minute stars revealed to his scrutiny. This would in any case be important, as Struve long since demonstrated, and as Herschel himself clearly recognised, as affording general evidence of the laws of stellar condensation ; but the comparison assumes a far greater importance when we remember what Herschel demonstrated as to the generally insulated nature of those aggregations which lie along the Milky Way streams. Let the meaning of his result be rightly appreciated; let it be clearly remembered *that he had in effect proved the Milky Way to consist in the main of real clouds of stars arranged in real streams in space*, and we see at once that to prove that these clouds are richer, both in minute stars and in relatively bright stars, than surrounding regions, is to prove the existence within each of them of widely different orders of real star magnitude. When we add the consideration that the Herschels, with their most powerful telescopes, were in some cases unable to resolve the richer regions of the Milky Way, we begin to recognise something of the complexity and variety of structure existing where uniformity of structure and variety of distance had been imagined.

It is obvious that what has thus been recognised respecting the system of stars is in perfect accordance with what has been ascertained respecting the distribution and nature of the nebulæ. So long as we assumed the stellar system to be of a tolerably uniform nature throughout its extent, there was some difficulty in admitting as probable the existence within that system of structures so distinct as the nebulæ, whether resolvable, irresolvable, or gaseous. Even the evidence afforded by the Magellanic Clouds as to the nature of all the orders of nebulæ, convincing though that evidence really is, did not impress the truth on the minds of those who yet could point to no flaw in the reasoning. The relations indicated by my equal-surface charts of the nebulæ, though equally satisfactory as evidence (besides being independent and therefore the more convincing), were similarly inoperative; and I think the beautiful and, in my judgment, most valuable maps by Mr. Waters which appear in the present volume, would not carry conviction of themselves.

But I submit that when we perceive the variety of stellar structure and aggregation, as well within the Milky Way as in extra-galactic regions, the evidence respecting nebulæ is brought into such perfect accordance with the evidence respecting stars that its real significance can no longer be misapprehended.

Here, moreover, the common evidence given by stars and nebulæ (agreeing also perfectly with that given by the Magellanic Clouds) is strengthened by the evidence derived from *details* of structure in the Milky Way, on its borders, and outside of it. I cannot here do more than allude to the nature of this evidence. I would note, then, that the balance of attractions within the Milky Way which Sir W. Herschel pointed out, suggests a reason as well for the paucity of irresolvable nebulæ within its bounds, as for the number of irregular clusterings and ordinary clusters contained within it. As Sir W. Herschel pointed out, the clusters in and near the Milky Way may be looked upon 'as so many portions of the great mass drawn together by the action of a clustering power of which they tend to prove the existence.' But this clustering power has been interfered with and checked by the balance of attractions. Outside the bounds of the Milky Way, a clustering power uncontrolled by any such cause has been at work, and to its influence may be ascribed those closely set nebular clusters which are either irresolvable or can else only be resolved with very powerful telescopes.

The gradual diminution in the number of clusters as we leave the galactic zone, and the equally gradual increase in the number of irresolvable nebulæ, show the reality of the conclusion here indicated. There is an even more convincing though more delicate proof, in the continuous change of character of the nebulæ from the most scattered clusters of the Milky Way to the absolutely irresolvable nebulæ of extra-galactic regions. Of course the actual constitution throughout the whole heavens, has in part depended on the distribution of star material, and not on position only. We see the influence of this in the charts of stars and nebulæ in Virgo and Coma at pp. 168 and 170, and on a wider scale in Mr. Waters' beautiful charts, where we notice how as irresolvable nebulæ become scarce, scattered clusters appear, not only on the heavens regarded as a whole, but in the several parts of the extra-galactic nebular systems and streams.

The evidence respecting nebulæ known to be gaseous points the same way; but it may be desirable to wait before summing up this part of the evidence until the spectroscope has been more widely and systematically applied to the nebulæ.

It is perhaps hardly necessary to insist on the evidence which the discussion of stellar proper motions can afford on questions relating to the structure of the universe. The general tendency of star motions, and the average magnitude of such motions, would supply evidence of extreme importance towards the discrimination of stellar aggregations in particular regions of space.

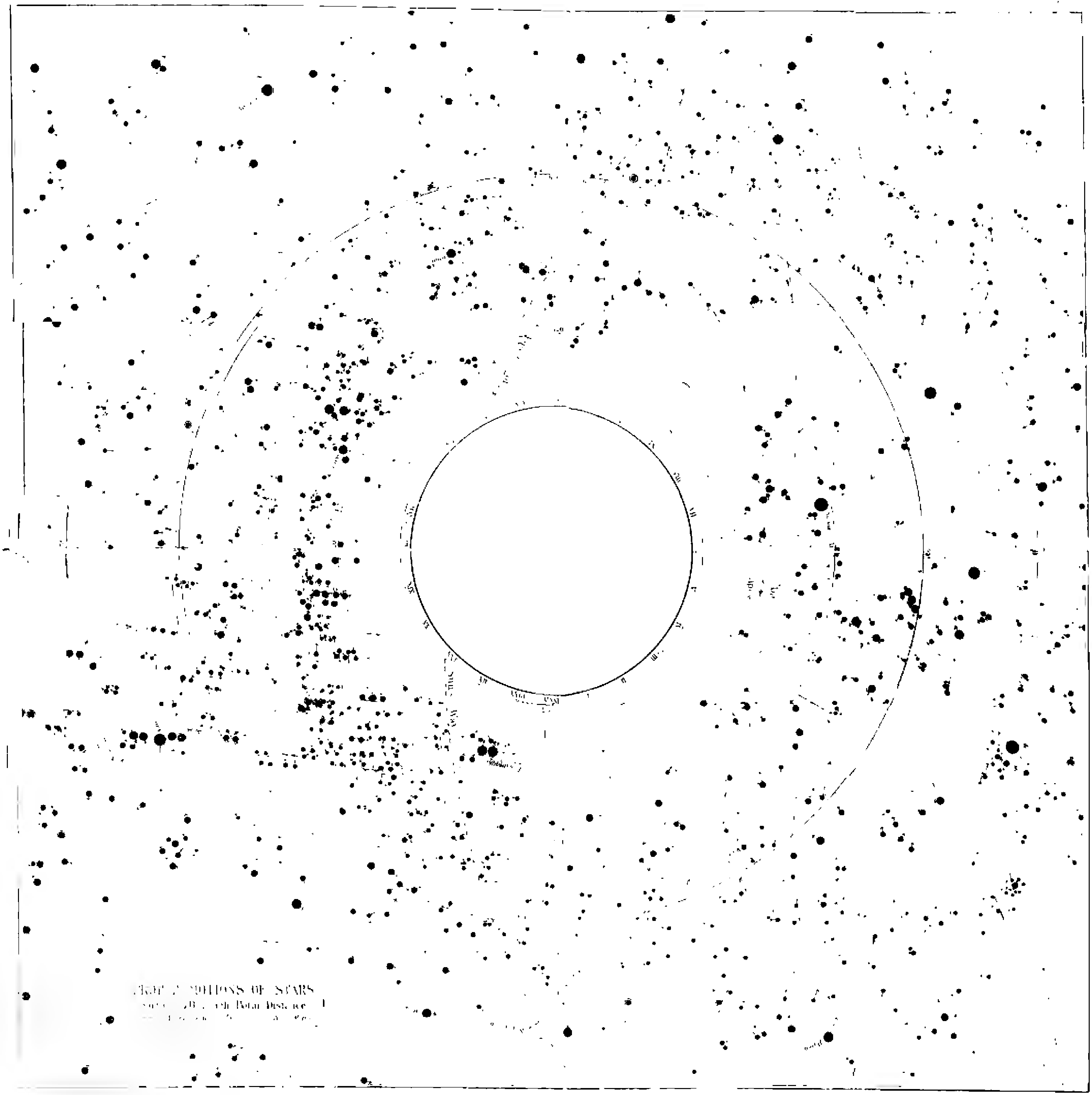
And here I must point out two mistakes which have been made by persons who have spoken of the phenomenon which I have called 'star-drift.'

One mistake is the confounding of this phenomenon with the star-drift due to the Sun's motion in space, which drift was first recognised by Sir W. Herschel and has since been more widely recognised by several other astronomers. This star-drift is general, but is seen only in the average of stellar motions. The star-drift which I have indicated is local, and has a different significance altogether. Before I had recognised it, I suggested its probable existence. When I had recognised it in several regions of the heavens, as in Gemini, Ursa Major, and so on, I pointed out how my theory on the point could be tested by the spectroscope, and selected the drift in Ursa Major as specially suitable for the purpose. I described the results which should follow if my theory were sound, and as is now well known my expectations were confirmed to the letter. The star-drift thus demonstrated in Ursa Major is a phenomenon perfectly distinct from the star-drift due to the Sun's motion. It takes place, as my maps of the stellar proper motions show, in almost exactly the reverse direction from that due to the Sun's motion. Moreover the real interest of the phenomenon consists, not in any evidence it gives as to the general motions of the sidereal universe, but in the fact that it marks out the stars affected by it as forming a subordinate system separated from other star-regions by starless spaces of enormous extent. Moreover the recognition of star-drift, especially if accompanied by spectroscopic evidence as to star structure, will probably become a potent means of ascertaining the architecture of various parts of the stellar universe.

The other mistake resides in the supposition that at the present stage of the inquiry it would be desirable for me to examine and discuss those observations which promise to afford more complete information respecting the proper motions taking place within the star system. I can only deal with wide and general relations; and the charts of proper motions already constructed by me suffice for that purpose, since their construction has led to the general theory of star-drift which it was chiefly important to establish. Anyone who may have leisure for the work can now examine suitable star-catalogues for the recognition or correction of estimated proper motions, with the assurance that such work will reap a rich reward. But for my own part the task I have taken in hand is of another nature, and will permit of no expenditure of time on details, however interesting the subsidiary evidence which such details may afford. I wish rather to set others at work on the various parts of the wide subject of the constitution of the heavens than to enter myself on work of detail. The structure of the star-system as a whole, or at least of the portion within our scope, is the subject to which I address myself, and too minute a discussion of the subordinate details would impair rather than improve the general view which I desire to obtain. *The proportions of a great structure are not favourably studied by approaching as near as possible to some particular part of it.*

Here my discussion of the subject of the constitution of the stellar heavens comes to a close so far as these pages are concerned. I take the opportunity of thanking students of astronomy, generally, for the patience and attention with which my views have been received. I trust and believe that those who have really examined the evidence I have adduced, will not have thought me dogmatic in the expression of opinions which I have based on a very thorough investigation of all the available evidence. I have been more than satisfied with the support I have already received. From Sir John Herschel in particular, who perhaps alone of all our leading astronomers, has taken special interest in the subject of the constitution of the heavens, I received encouraging expressions of opinion (which will one day be published), important not only in their relation to the researches I communicated to him, but in their prospective bearing on researches which have been completed since his lamented demise. For the rest, it is not to be expected, and, in fact, it is not possible, that general attention should be drawn to a subject so far removed from ordinary astronomical studies, whether practical, observational, mathematical, or theoretical. At all times in the history of Astronomy, the number of those who have been attracted to the study of the laws of stellar distribution has been small, though, strictly speaking, the very name Astronomy implies such study more specially than any other now recognised subject of astronomical research. But the true student of science is content if he 'fit audience find though few.' The investigation and demonstration of the truth are more important than the mere numerical array of supporters. Though, indeed, if on the one hand, as I had expected, few have been attracted to a subject of great difficulty, yet, on the other, I have had small occasion to complain of cavil or objection: and, if the results to which I have been led, or may be led hereafter, are just, it is a matter of indifference to me whether they be generally accepted at a late or early season, in my own lifetime or not till long afterwards.

From the *Monthly Notices of the Astronomical Society* for October 1873.



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